Resource Cursed or Policy Cursed?

U.S. Regulation of Conflict Minerals and the Rise of Violence in the Congo[*]

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Abstract: There is widespread belief that violence in poorly governed countries is triggered by international demand for their natural resources. We study the consequences of U.S. legislation grounded in this belief, the "conflict minerals" section of the 2010 Dodd-Frank Act. Targeting the eastern Democratic Republic of the Congo, it cuts funding to warlords by discouraging manufacturers from sourcing tin, tungsten, and tantalum from the region. Building from Mancur Olson's *stationary bandit* metaphor, we explain how the legislation could backfire, inciting violence. Using geo-referenced data, we find the legislation increased looting of civilians, and shifted militia battles towards unregulated gold mining territories. These findings are a cautionary tale about the possible unintended consequences of boycotting natural resources from war-torn regions, and the use of international resource governance interventions.

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1. Introduction

The belief that poorly governed countries are cursed by their natural resources is pervasive and it sometimes influences international policies towards trade in resources with those countries. When a country is afflicted by the rawest form of the curse, increased international demand for its resources leads to civil conflict rather than improved human welfare. International demand is a curse because it generates revenue to fight over, and because it provides money for weapons and soldiers (see Hirshleifer 1991, Grossman 1991, Collier and Hoeffler 2004, Olsson 2007, Janus 2011).

In this paper, we study a region that is ostensibly the quintessential example of a place afflicted by the resource curse: the eastern Democratic Republic of the Congo (DRC). The DRC is the world's 11th largest country by area and the 19th most populated. It ranks 215th out of 216 countries in national assessments of human rights protections, 156th out of 162 countries in assessments of peacefulness, and dead last in GDP per capita.[1] The eastern DRC contains minerals that supply surging world markets for mobile phones, tablets, flat screen televisions, and other modern electronic devices. The region is rich in natural resources, but poor by every conceivable measure of human welfare.

It is clear that some revenue from mineral sale goes to warlords who control aspects of the trade and sometimes commit brutal acts of violence, which contributes to the widespread perception that the eastern DRC is cursed by surging global demand. This is why human rights advocacy groups have dubbed the endowments "conflict minerals", which officially refers to tin, tungsten, tantalum, and gold from the region.

Convinced that the DRC suffers from the resource curse, advocacy groups, such as the Enough Project and Global Witness, have successfully lobbied for top-down policies to reduce international demand for Congolese minerals extracted with armed group involvement. The primary policy is the U.S. Dodd-Frank Act of 2010, Section 1502, which was endorsed by co-sponsor Barney Frank as a measure to "cut off funding to people who kill people" (Aronson 2011). Section 1502 regulates large U.S. manufacturing companies whose products may contain conflict minerals by requiring them to trace and report the origins of minerals it uses. Section 1502 has acted as an "intended or unintended boycott" on purchases of tin, tantalum, and

[1] See www.ihrri.com/contry.php, www.visionofhumanity.org/#/page/indexes/global-peace-index, and data from the World Bank at http://data.worldbank.org/indicator/NY.GDP.PCAP.CD.

tungsten – the "3Ts" - from the eastern DRC (Pöyhönen et. al 2010, 27). Gold, however, has been de facto exempt because it is more difficult to trace its origins as we explain in section 2. Two years after Dodd-Frank was passed, advocacy groups were claiming success, stating "the passage of conflict minerals legislation … have helped lead to a 65% drop in armed groups' profits from trade in tin, tantalum, and tungsten …" (Johnson 2013, p. 53).

But did the legislation actually reduce violent conflict? In this paper, we examine the effects of Section 1502 on the incidence of conflicts in the eastern DRC. We theorize on mechanisms through which the policies could backfire in the short-run, causing violence near mining sites to increase rather than fall. We assess the theory by employing geo-referenced and detailed data on militarized mining sites and on armed conflict before and after the policies, from 2004 through 2012. The evidence suggests the legislation significantly increased the incidence of looting and the incidence of violence against civilians by at least 291 and 143 percent respectively. Our quantitative findings contribute to the ongoing debate about Section 1502 and support qualitative claims that the law has harmed civilians in artisanal mining regions (Aronson 2011, Sematumba 2011, Pöyhönen et. al. 2010, Seay 2012). The findings also contribute to the debate about resource governance interventions in general, such as the Kimberley Process Certification Scheme, that may rely on "…unsupported assumptions regarding how natural resources are linked to the motivations of combatants" (Cuvelier et al. 2014, 2).

In studying what went wrong, we employ an analytical approach that complements but differs from other studies of how shocks to the value of resource endowments affect violence in countries with weak property rights. That literature is predominantly concerned with two competing effects: rapacity and opportunity cost. On one hand, a rise in endowment values means there are larger spoils to fighting, so the rapacity effect implies an increase in violence in response (see Hirshleifer 1991, Grossman 1991, Olsson 2007).[2] On the other hand, positive shocks to resource values could increase the opportunity cost of fighting, drawing labor into non-conflict industries (see Becker 1968, Grossman 1999, Chassangy and Miquel 2009).

[2] Recent empirical support for the dominance of the rapacity effect is provided by Angrist and Kugler (2008) who find a positive relationship between coca prices and violence in Columbian municipalities dependent on coca production. Besley and Persson (2008) also find a positive relationship between world market prices of a country's main commodities and civil wars using a broad sample of countries. Also consistent with rapacity effect is the finding that U.S. food aid has increased the incidence and duration of civil conflicts, particularly in countries with a history of civil conflict, presumably because the food aid creates spoils to fight over (Nunn and Qian 2014).

Focusing on these theoretical effects suggests the key to understanding whether violence rises or falls with resource-value shocks lies in understanding the extent to which resource commodity production is labor rather than capital intensive (Dube and Vargas 2013). In cases where production is labor intensive (e.g., agriculture), the opportunity cost effect can dominate but in cases where production is capital intensive (e.g., oil extraction), the rapacity effect can dominate.[3] Mining in the eastern DRC is labor intensive, with almost no capital inputs (see section 2). This fact suggests the opportunity cost effect may dominate in our setting. If it does, violence should have increased as a result of Section 1502, which lowered the net value of mineral endowments to local civilians and militias. Thus, opportunity cost is one candidate explanation for our empirical findings.

The opportunity cost concept, however, does not obviously explain patterns of conflict that we observe in the data. First, the policies primarily increased looting and violence against civilians, rather than increasing militia battles, and it is not clear to us why falling opportunity costs would have this asymmetric effect. Second, to the extent the policies increased battles between militia groups, they did so in gold mining territories rather than elsewhere in the eastern DRC. This nuance is not explained by blanket decreases in the opportunity cost of militia participation; it is better explained by rapacity to fight over unregulated gold.

Rather than relying on rapacity and opportunity cost tradeoffs for theoretical guidance, we develop a simple analytical framework that is related to crime displacement models and inspired by research on the economic functions of organized crime (Skaperdas 1992, 2001) and Olson's (1993) *stationary bandit* metaphor.[4] Stationary bandits are akin to Mafia groups that tax neighborhoods or industries. They emerge in power vacuums, where the state is absent, to provide protection (Skaperdas 2001). Mafia groups maximize revenues by selling protection to civilians – both against crime they will commit themselves (if not paid coerced taxes) and against crime committed by others. Hence, if one Mafia group has the power to monopolize

[3] Dube and Vargas (2013) find a positive relationship between oil prices and conflict in oil producing regions of Columbia, but a negative relationship between coffee prices and conflict in coffee producing regions. Similarly, Brückner and Ciccone (2010) find that decreases in a country's main commodity are associated with increases in the likelihood of civil war, which is consistent with a dominant opportunity cost effect.

[4] Our theoretical reasoning is informed by Mampilly's (2011) research on *Rebel Rulers*. Our reasoning complements that of Maystadt et. al (2014), who model the incentives of armed groups to exploit and protect mineral resources in the DRC using ideas from the crime displacement literature, and to Sanchez de la Sierra (2014), who also analogizes armed groups in the DRC to stationary and roving bandits.

taxing, there will be little to no ordinary violence, except for the occasional brutal act committed as a credible display of authority or perhaps for reputation building (Konrad and Skaperdas 1997, Silverman 2004). Yet this low-violence, stationary bandit equilibrium is precarious. Mafia groups must find it advantageous to protect rather than harm civilians, and to remain in a neighborhood or industry rather than moving to loot other unprotected neighborhoods or to challenge the areas controlled by competing groups.

Applying this analogy to the eastern DRC, mafia groups are militias, mining villages are neighborhoods, and the relevant industries are 3T and gold mining. As we describe in section 2, the mafia characterization fits our setting, where there is clearly a power vacuum created by the lack of state authority. We refer to detailed surveys of militarized mining sites, which describe how the sites were controlled by "mafia-like" groups who "taxed" labor in somewhat predictable ways, prior to Dodd-Frank. In exchange, the armed groups provided a crude form of protection.

For these reasons, our analytical framework in section 3 assumes that militia groups choose between stationing their armed capital at mining sites for the purpose of taxing, roving and looting civilians, and challenging another militia group for control over mineral deposits. Dodd Frank sharply lowered the value to militias of stationing their soldiers at 3T sites, but had less of an effect on the value of taxing labor at gold mining sites. We explain how this asymmetric shock could have triggered a sequence of violent looting, first against civilians near 3T deposits as militias ceased 'protection' and then towards non-mining areas. Logically, the asymmetric regulation of 3T and gold mining would lead to armed group competition for control over gold deposits. We examine these detailed ideas empirically in section 5, in addition to assessing the more general effects of the policies, and we describe alternative explanations the one could apply to the observed empirical patterns in section 6.

2. Background

A. Artisanal Mining Prior to Dodd Frank

Mining has been an important contributor to the DRC economy since colonial times. According to the U.S. Geological Survey, the mining sector's recorded contribution to GDP was 13.4 percent in 2009 but the World Bank estimates that it could account for 20-25 percent if the sector was better managed (World Bank 2008). Mineral deposits are scattered throughout the eleven provinces, but it is in the east where artisanal mining is infused with armed groups and where the attention on conflict minerals is focused. The eastern provinces usually associated with conflict minerals are North and South Kivu, Maniema, Orientale, and Katanga (map 1) (Bawa 2010, D'Souza 2007, de Koning 2011).

Artisanal miners are not officially employed by mining companies but instead work independently using their own resources to pan and dig for alluvial, open pit, and hard rock mineral deposits.[5] Artisanal mining is labor intensive and employs minimal technological inputs.[6] Estimates of the number of artisanal miners in the five eastern provinces are rough but ranged from 710,000 to 860,000 in 2007 (D'Souza 2007). The World Bank (2008, 10) estimates that artisans produce 90 percent of the minerals exported from the country.

The key minerals produced by artisanal miners in the eastern provinces are tin (from cassiterite), tantalum (from coltan and tantalite), tungsten (from wolframite) and gold. The Enough Project estimates the DRC's contribution to world supply of the 3Ts and gold. For tantalum, the estimate is 15-20%; for tin 6-8%; for tungsten 2-4%; and for gold, less than one percent. However, tin and gold have generated much more local revenue.[7]

Map 2 shows the distribution of artisanal mining sites based on interactive maps created by the International Peace Information Service (IPIS) during 2008-2010, before Dodd-Frank was passed. The data were gathered by teams of researchers equipped with GPS devices and questionnaires. The GPS devices identified the geographic coordinates of sites and the questionnaires solicited information about mineral resources, the number of workers, and the presence of armed militia groups and their interactions with miners. The IPIS outsourced data collection to local teams with knowledge of specific DRC territories in order to best locate and access remote mining sites.[8] In spite of this strategic methodology, the IPIS admits that their mapping is "not an exercise in exact science" (Spittaels and Hilgert 2009, 7) because logistical

[5] According to the 2002 DRC Mining Code, artisanal mining is "any activity by means of which a person of Congolese nationality carries out the extraction and concentration of mineral substances using artisanal tools, methods and processes, within an artisanal exploitation area limited in terms of surface."

[6] The lack of capital investment is almost certainly due in part to the insecurity of property rights to mining sites (see Bohn and Deacon 2000).

[7] www.enoughproject.org/files/Comprehensive-Approach.pdf. The Enough Project also estimates that tantalum has provided armed groups in the eastern Congo with $12 million in 2008, tungsten with $7.4 million, tin with $115 million, and gold with $50 million.

[8] This allowed teams to "… rely on their own networks and contracts in the region, which allowed them to enter sites that others could not and speak to people who would never answer the same questions asked by a stranger, especially a white man (Spittaels and Hilgert 2009, 6). The drawback is that the use of multiple researchers led to more "…inconsistencies due to the different 'research habits' developed by the teams" (p. 6).

challenges prevented teams from locating and entering every mining site.[9] Rather than omitting important mining sites that its researchers could not physically visit, the IPIS estimated those mining locations and included them in the maps.

With these caveats in mind, we turn to map 2. It suggests that artisanal mining sites follow geological endowments rather than simply following the infrastructure of road networks, reducing concern that the distribution of reported mines is biased towards infrastructure. The coincidence of mining sites with permanent rivers suggests a prevalence of alluvial mineral deposits across the region. Table 1 shows that gold and tin mines dominated the landscape and that coltan and wolframite sites were relatively rare. Approximately one-half of the mines were controlled by, or visited regularly by, armed militias (including the Congolese Army) usually for the purpose of taxing civilian miners.[10]

Table 2 summarizes information about armed group control of mining sites, prior to Dodd Frank. Examples of interactions between these groups and miners, from Spittaels and Hilgert (2008, 2010), include the following: "The soldiers secure the area and levy taxes"; "12 soldiers are present. Anyone entering the site has to pay them FC 500 to 1000"; "The FDLR tax the miners"; "The miners have to pay the FRF for the authorisation to exploit (land rights)"; "Receive 500 FC from each miner on Thursdays and Fridays; and "The Mayi-Mayi tax the miners, they hire people to work for them and some of them mine themselves." Note that in these examples, and in most others, the taxes were per-laborer fees rather than taxes on production, perhaps because this structure lowered monitoring costs. We stylize our section 3 analytical framework around this data pattern, by modeling taxing on a per miner basis.

B. Dodd-Frank and the DRC Mining Ban

The United States' first legislative attempt to regulate conflict minerals was in April 2009 with the proposed Congo Conflict Minerals Act. That legislation failed to pass but its

[9] Spittaels and Hilgert (2009) list three logistical challenges. These are: many mines are remote and accessible only by foot; mining activity can shift from one place to another; and security concerns can prevent teams from traveling freely. These challenges are highlighted in a U.S. Department of State Map of conflict mining areas, which concludes that the IPIS maps are not entirely comprehensive in their coverage of mining sites (see https://hiu.state.gov/Products/DRC_MineralExploitation_2011June14_HIU_U357.pdf).

[10] The minerals are transported to buyers located in the border cities of North and South Kivu but also in Orientale and Katanga (United Nations 2011). The minerals are often exported through Rwanda and shipped to Asian smelters before returning to the U.S., Europe, and Asia as components in electronic devices (De Koning 2011).

fundamental goals were carried through as Section 1502 of the Dodd-Frank Act.[11] The central purpose of Section 1502 is to discourage the use of conflict minerals by major manufacturing and processing companies. It also authorized Congress to produce, and make public, a map of mineral-rich zones and illegal armed groups in the eastern region of the DRC. The map was commissioned by the IPIS as discussed above. The map identifies militarized mining sites in North Kivu, South Kivu, Maniema, and their "hinterlands" which extend into a few territories in Orientale and Katanga.

Section 1502 affects the reporting requirements of perhaps half (at least 6,000) of all publicly traded companies in the United States (KPMG 2011). It directs the Securities Exchange Commission (SEC) to make disclosure rules for companies manufacturing products containing tin, tungsten, tantalum (i.e., the "3Ts") or gold. The rules require companies to conduct "due diligence" on the origin of minerals; if the origin is from the DRC conflict mining zones then companies must report on the possibility that warlords have benefitted from the purchases.

The Dodd-Frank Act was signed into law on July 21, 2010 (figure 1). Although the Act did not prohibit the purchase of minerals from eastern DRC conflict zones, many observers say it acted as a swift de facto boycott of minerals from the DRC (Pöyhönen et. al. 2010, Seay 2012). This is because the easiest way for companies to report being conflict-free is to avoid mineral sources from the entire region. The boycotting of eastern DRC minerals has been more explicit since April 1, 2011, when a coalition of large electronics and high-technology companies – the Electronic Industry Citizenship Coalition (EICC) - stopped buying the 3Ts from smelters unable to prove their source minerals did not fund DRC conflict (Wimmer and Hilgert 2011).

Probably as a response to Dodd-Frank, the DRC also imposed a governmental ban on artisanal mining on September 11, 2010.[12] The ban covered three provinces - Maniema, North Kivu, and South Kivu (see figure 1). A week after the ban was announced, the Congolese Minister of Mines stated that it concerned extraction of the 3Ts (see de Koning 2010) but observers note considerable confusion about whether or not gold was covered and many reports

[11] See www.opencongress.org/bill/111-s891/show.

[12] DRC's President stated the ban's goal was to weed out "mafia groups" from the mining industry. Many observers think the ban was a response to international pressure to stop trade in conflict minerals (Geenan 2012, Seay 2012). Extraction of the 3Ts was to be halted immediately but traders thought they had until October 15 to sell mineral stocks to exporters and exporters thought they had until November 15 to arrange export (see De Koning 2011).

include artisanal gold among the banned mines. The ban was lifted on March 10, 2011, shortly before the international EICC boycott took hold (de Koning 2011).

How did Dodd-Frank and mining ban impact mining output? Official data reveal a large drop in exports of tin, coltan, and wolframite during 2011-2012. Figure 2, panel A shows the decrease in tin exported from North Kivu, South Kivu, and Katanga, the tin producing provinces of the DRC. The volume of official exports tracked the world price from 2004-2009, but then dropped significantly during 2010-2011 as the world price continued to rise. Some of export decrease was offset by increased exports from Katanga Province, which was exempt from the mining ban and largely outside of the conflict territory mapped by the U.S. State Department. Panel B shows monthly export data of tin from North Kivu. Official exports went to zero during the ban. Stockpiles were exported in March 2011, during a window of time between the end of the ban and the April 1, 2011 deadline that EICC companies had set to stop buying 3T minerals from smelters lacking traceability systems. Chinese companies continued to buy 3T minerals from eastern Congo but at prices discounted of "up to 80 percent compared to world market valuations" (Carisch 2012, 15, see also Johnson 2013).

Export data are a less reliable indicator of gold production because approximately 98 percent of gold mined in the eastern DRC was smuggled, both before and after the Dodd-Frank Act (see, e.g., United Nations 2014, de Koning 2011).[13] Panel B shows USGS estimates of gold production over 2004-2012. It decreased slightly during 2011 when the mining ban was in force, but gold production rose during 2012 in spite of Dodd Frank. Why did gold production rise despite its official status as a "conflict mineral" under Dodd Frank? There are two main reasons. First, DRC gold mainly goes to the Middle East and East Asia to supply jewelry markets there, whereas 3Ts are primarily consumed by electronics companies that are members of the EICC or regulated by Dodd Frank (de Koning 2011). Second, while it is technologically feasible to track the origin of 3T minerals and demonstrate whether or not their origin differs from those controlled by armed groups, this is less easily accomplished for gold. The problem is that it is relatively easy to co-mingle gold from one eastern DRC site with gold from another source early in the supply chain process. This is because gold is more easily smelted (i.e., separated from the ore) on site or earlier in the supply chain, in part because gold is often panned in rivers instead of

[13] The United Nations (2014) reports that networks engaged in smuggling gold from the Democratic Republic of the Congo through neighboring countries are more than 20 years old, and deeply entrenched. Gold is easier and more profitable to smuggle than the 3Ts because it is much more valuable by weight.

extracted as ore and in part because gold has a lower melting point meaning that less equipment and energy is needed to smelt it. These geological facts make it very difficult to track specific gold consignments from mine to end user (see Prendergast and Lezhnev 2009, Schraeder 2011).

To corroborate and cross-reference the patterns of 3T and gold production implied by export data and USGS estimates, we have assembled satellite images of forest cover over 2000-2012 from Hansen et al. (2013). Following Wimmer and Hilgert (2011), who study aerial images of a large tin mine in North Kivu, we compare rates of deforestation around the 659 mining sites (see map 3) before and after Dodd Frank. We assume that accelerating deforestation within a small radius around the centroid of a mine implies increased mining. We assume that declining deforestation around a mine implies slowing mining.

We define relative deforestation for a radius around each mine in the following way.

$$\text{deforestation} = \begin{Bmatrix} -1 & \text{if 2011 + 2012 forest loss} < 2008+2009 \text{ forest loss} \\ 0 & \text{if 2011 + 2012 forest loss} = 2008+2009 \text{ forest loss} \\ +1 & \text{if 2011 + 2012 forest loss} > 2008+2009 \text{ forest loss} \end{Bmatrix}$$

We evaluate the relationship between Dodd Frank and deforestation with regression equation (1).

$$\begin{aligned} (1) \qquad & \mathit{deforestation}_m = \alpha + \lambda(3T \text{ indicator})_m + \delta(\text{policy territory indicator})_m + \\ & \beta(3T \text{ ind.} \times \text{ policy ind.})_m + \eta(\text{forest loss over 2001-2007})_m + \varepsilon_m \end{aligned}$$

Here m indicates the 659 mining sites. The policy territories are displayed in map 3 and defined in table 4. The 3T indicator equals 1 if the mine is a 3T mine. The omitted category accounted for in α is gold mines in non-policy territories. We control for aggregate deforestation over 2001 to 2007 to account for differences in forest cover at the beginning of 2008.

Table 3 reports OLS estimates for radius lengths of 100, 200, and 500 meters. The key results are the estimates of β, which are negative and statistically precise, indicating that the conditional mean of the dependent variable is smaller around 3T mines in areas targeted by Dodd Frank. These negative coefficients are consistent with 3T export data, and with on-the-ground anecdotes, in that all pieces of information suggest Dodd Frank was successful in slowing 3T mining in targeted areas. The positive estimates of λ provide some evidence that 3T mining increased outside of the policy territories, which is consistent with the increase in reported exports from Katanga province after Dodd Frank (see figure 2). The coefficients on δ are statistically imprecise and effectively zero, which is consistent with other evidence above suggesting that Dodd Frank did not slow gold mining within the areas targeted by Dodd Frank.

To summarize, disparate data sources suggest that Dodd Frank was effective in slowing 3T artisanal mining within the areas targeted by Section 1502. In the next section we theorize about how this policy disruption affected conflict involving armed groups.

3. Analytical Framework

We present a stylized analytical framework to guide the empirical analysis and to highlight some channels through which Dodd Frank could have increased conflict. We impose several simplifying assumptions to facilitate a concise explanation of the channels and, where possible, our assumptions are aligned with the facts about the empirical setting just described.

The simple framework is intended to capture the essence of Olson's (1993) stationary bandit idea and theories of organized criminals (Skaperdas 2001) and the structure closely resembles crime displacement models as we explain in section 6. In order to focus on short-run policy impacts, we fix the amount of warring capital held by armed groups and describe the conditions under which it will be 'stationed' at mining sites for the purpose of taxing mining labor. Focusing on a two-period planning horizon, we explain how the policies could disrupt a "stationary bandit" equilibrium, triggering an increase in looting and battles.

A. Labor and Mineral Extraction

We imagine a landscape containing N homogenous agricultural regions (A) and two artisanal mining regions. The mining regions contain one of two different minerals, with i = G (for gold) or C (for cassiterite).

There is a fixed number of homogenous civilian laborers, L, who work in one of the regions such that $L = L_A + L_G + L_C$. We assume that agricultural labor spreads out evenly across the regions so that $L_A = \sum_{n=1}^{N} L_{A,n} = NL_{A,\mathrm{j}}$ where j signifies any one region. The expected wage in agriculture is fixed at w, and is effectively a subsistence wage. Random rainfall shocks occur in the different regions such that the realized wage during a time period is high ($\overline{w}$) in some regions and low ($\underline{w}$) in others with $\overline{w} > w > \underline{w}$. Artisanal mining technology is such that the quantity extracted, Y_i, increases with the number laborers but at a decreasing rate so that

$dY_i / dL_i > 0$ and $d^2Y_i / dL_i < 0$. To simplify exposition we assume $Y_i = \sqrt{L_i}$ and ignore dynamic depletion of the mine.[14] Miner net income is $p_i \sqrt{L_i} / L_i - \tau_i$ which we rewrite as $p_i \big/ \sqrt{L_i} - \tau_i$.The variable $p_i > 0$ is the price per unit of mineral output and $\tau_i \geq 0$ denotes a per-miner tax.

B. Militia Groups

There are several armed groups in the Eastern DRC but we assume two groups for simplicity. Each group has a fixed amount of armed soldiers, *S*. We assume this armed capital is specialized towards war-making, protecting and taxing civilians, and looting civilians. It is not used for mining.[15] If the groups battle one another, the probabilities of victory are determined by relative size and known ex ante, for example *P*(Group 1 defeats Group 2) = $S_1 \big/ (S_1 + S_2)$.

Each militia group ultimately chooses to deploy their soldiers into one of two mutually exclusive activities: either stationing in a region for the purpose of taxing civilians or roving across regions for the purpose of looting. Looting is a surprise event that takes all income from a region's civilians whereas taxing is predictable, it does not strip civilians of all income, it is effectively consensual (because civilians can move to untaxed regions), and it forecloses looting by the other armed group. Battles between armed groups occur only to the extent that the two groups seek control over the same region. The maximum value to an armed group of taxing a mining region is given by

$$(2)\quad V_i^{TAX} = \sum_{t=0}^{T} \rho_i^t [\tau_i^* L_i (p_{i,} w,\ \tau_i^*)].$$

Here *T* represents the planning horizon, τ_i^* is the revenue-maximizing tax on each laborer, L_i^* is the number of laborers, and ρ is the discount factor, with $0 < \rho < 1$.[16]

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[14] Although one could analyze a more general concave function of *L*, using this specific functional form is useful because it yields explicit expressions that are easy to compare and interpret.

[15] Guns are not productive inputs in mining but the soldier could in principle employ his labor in mining. We abstract from this possibility, however, by assuming soldiers can always earn a higher return on their labor in non-mining activities. There is evidence that most members of armed groups in the Eastern DRC do not regularly mine themselves (see Sanchez de la Sierra 2014) but there are exceptions described in the IPIS mapping surveys.

[16] We assume that militia groups tax labor rather than output because the IPIS data described in section 2 indicates that labor taxes were typically used at militarized mining sites, whether gold or the 3Ts. In some presentations of discounting, including those in natural resource economics, it is assumed that $\rho = 1/(1+\delta)$ where $\delta > 0$ is the discount rate (see, e.g., Conrad 2010).

A region is secured for taxing if two conditions hold: 1) the armed group has stationed its soldiers in the region and 2) it is not engaged in battle with the other armed group.[17] Stationing soldiers 'protects' labor from being looted by the other armed group. We assume that neither group has sufficient armed soldiers to station at and secure more than one region.

The incentives for an armed group to station in the mining region, rather than to rove and loot, depend in part on mineral prices (p_i) relative to the favorable agricultural wage ($\overline{w}$) as we explain below. The random nature of realized wages across agricultural regions makes those regions targets for looting and ill-suited for taxation. This is because armed groups can plausibly extract more revenue from roving and looting particular agricultural regions as they experience favorable shocks, rather than stationing in a region through periods of unfavorable shocks. [18] For this reason, we rule out the taxing of agricultural regions in the analysis that follows.

C. Stationary Bandit Equilibrium

The system is in a stationary bandit equilibrium if each militia group is 'stationed' in a separate mining region, taxing labor, and neither group could gain expected revenue by looting civilians, or by challenging the other armed group for control over the other mining region. If we assume that *L* is large enough to dissipate mining rents, then a stationary-bandit equilibrium dictates that labor will enter each mining region until after-tax income per miner equals the expected agricultural wage such that

$$(3)\ \frac{p_i}{\sqrt{L_i}} - \tau_i = w \quad for\ i\ =\ G,\ C.$$[19]

The equilibrium tax revenue is given by

$$(4)\ \tau_i^* L_i = \tau_i^* \left(\frac{p_i}{w + \tau^*} \right)^2.$$

Solving for the revenue-maximizing tax rate, which necessarily considers the reaction by labor, we find $\tau^* = w$. Thus, equation (4) can be rewritten as

[17] These conditions are related to the conditions for secure 'ownership' of any economic asset (see Barzel 1997). Similar requirements were important in the evolution of property rights to mines in Australia, the United States, and elsewhere (see e.g., Anderson and Hill 1975, Umbeck 1977, La Croix 1992, and Libecap 2007).
[18] We assume commuting costs are negligible throughout this analytical framework.
[19] This expression assumes that laborers are risk neutral.

(5) $\tau_i^* L_i = \dfrac{p_i^2}{4w}$.

In a stationary equilibrium, labor is distributed as in (6), all laborers earn a subsistence wage in expectation, and there is taxing but not looting.

(6) $L_G = \dfrac{p_G^2}{4w^2};\ L_C = \dfrac{p_C^2}{4w^2};\ L_A = L - \dfrac{p_G^2 + p_C^2}{4w^2}$.

D. Incentives to Loot

We now consider the payouts to an armed group from looting and roving, assuming the stationary-bandit equilibrium as a starting point. To be more concrete, we assume Group 1 is initially in control of and taxing the cassiterite (tin) region, in period *t -1,* and Group 2 is initially in control of and taxing the gold region. We focus on Group 1's incentives and consider Group 2 only in the context of how it will respond to the actions of Group 1.

Figure 3 illustrates Group 1's incentive to loot in a one-period model. If Group 1 chooses to loot, by taking the subsistence earnings from tin miners, it accrues the one-time revenue earned in the tin mining region of

(7) $p_C Y_C = p_C \sqrt{L_C(p_{C,} w, \tau_C^*)} = \dfrac{p_C^2}{2w}$.

This revenue is twice the per-period revenue Group 1 could extract by taxing (equation 5). Hence, it would never be optimal for Group 1 to tax if $T = 1$.

There is a cost to looting the mining region when Group 1's planning horizon is two periods. We assume a mining region will be unproductive in t =1 if it was looted in t = 0 (because the looting event is violent and harms the labor force).

Figure 4 illustrates the decisions and payouts to Group 1 when it has a two-period planning horizon. If Group 1 taxes in t =0, then its soldiers remain stationed in the tin mining region through t =1. It will always be optimal for Group 1 to use those stationed soldiers to loot the tin mine during t = 1, because it is the final period.[20] The discounted revenue from taxing the mine in t =0 is therefore $(1+2\rho)\dfrac{p_C^2}{4w}$, the 'stationary path' revenue.

[20] The planning horizon resets after t =0, however, so the final period of looting would never actually be reached.

If Group 1 loots the tin region during t = 0, then its soldiers are free to rove and will either loot an agricultural region experiencing a favorable rainfall shock or battle for the right to loot the gold region in t =1. Consider first the payout from the 'looting path', which is $\frac{p_C^2}{2w} + \rho \overline{w} L_{A,n}$. The term $\overline{w} L_{A,j}$ represents agricultural revenue in any region where a favorable shock has occurred with $L_{A,j} = L - NL_{A,n \neq j} - \frac{p_G^2 + p_C^2}{4w^2}$ in period t – 1. To simplify, we assume labor is not mobile in or out of regions during the two-period planning horizon; laborers makes location decisions based on militia behavior in t - 1.

Given these assumptions, a sufficient (but not necessary) condition for Group 1 to loot the tin region in t = 0 is $(1+2\rho)\frac{p_C}{4w} < \rho \overline{w} L_{A,j}$. Assuming $\overline{w} = \alpha w$ with $\alpha > 1$ indicating the degree to which the weather shock raises wages above expectation, we rewrite the expression as

(8) $\rho\left(2 - \alpha L_{A,j}\frac{4w^2}{p_C^2}\right) < 1$.

Comparative statics of (8) indicate that looting is more likely with an increase in w or α, or with a decrease in p_C or ρ.

The attractiveness of the 'loot and battle' path to Group 1 depends on how Group 2 would respond to the observation of Group 1's looting in t = 0. If Group 2 would respond by looting the gold region in t =0, then Group 1 would have no incentive to battle for the gold region (which was rendered unproductive by the previous period looting). We explain the conditions under which Group 2 will respond by looting shortly.

If Group 2 would remain stationed to defend the gold region in t = 1, then the expected revenue to Group 1 of the 'loot and battle' path is $\frac{p_C^2}{2w} + \rho\pi\frac{p_G^2}{2w}$. The notation π refers to P(Group 1 defeats Group 2). Conditional on Group 2 staying to fight, Group 1 prefers the 'loot and battle' path to the 'stationary path' when $(1+2\rho)\frac{p_C^2}{4w} < \frac{p_C^2}{2w} + \rho\pi\frac{p_G^2}{2w}$, which we rewrite as

(9) $\pi\frac{p_G^2}{p_C^2} + \frac{1}{2\rho} > 1$.

Comparative statics of (9) indicate the ‘loot and battle’ path becomes more attractive to Group 1, relative to the ‘stationary path’, as the relative price of gold rises and the discount factor falls.

Again, conditional on Group 2 staying to fight, Group 1 prefers the ‘loot and battle’ path to the ‘looting path’ when $\frac{p_C^2}{2w}+\rho\pi\frac{p_G^2}{2w}>\frac{p_C^2}{2w}+\rho\alpha wL_{A,j}$, which we rewrite as

(10) $$\pi p_G^2 > 2\alpha w^2 L_{A,j}.$$

Comparative statics of (10) indicate the ‘loot and battle’ path becomes more attractive to Group 1, relative to the ‘looting path’, as the price of gold rises.

However, the ‘loot and battle’ path is feasible only if Group 2 remains stationed in the gold region during t =1 to defend it rather than looting the gold region in t = 0. Group 2 will remain stationed if $(1+2\rho(1-\pi))\frac{p_G^2}{4w} > \frac{p_G^2}{2w}+\rho\alpha wL_{A,j}$, where $1-\pi$ =P(Group 2 defeats Group 1). Rewriting, Group 2 will remain stationed and tax in t =0 if

(11) $$\pi \le 1-\frac{1}{2\rho}-\frac{2\alpha w^2 L_{A,j}}{p_G^2}.$$

Comparing (9), (10), and (11) indicates that, although a larger π raises the expected payout to Group 1 of the ‘loot and battle’ path - conditional on Group 2 staying to fight - a larger π simultaneously lowers the probability that Group 2 will stay to fight. Rewriting, we note that battle over the gold region will occur if conditions (12) and (13) both hold.

(12) $$\left(1-\frac{1}{2\rho}\right)\left(\frac{p_C}{p_G}\right)^2 < \pi \le 1-\frac{1}{2\rho}-\frac{2\alpha w^2 L_{A,j}}{p_G^2}.$$

(13) $$\frac{2\alpha w^2 L_{A,j}}{p_G^2} < \pi \le 1-\frac{1}{2\rho}-\frac{2\alpha w^2 L_{A,j}}{p_G^2}$$

An increase in the price of gold increases the probability that conditions (12) and (13) will hold. A decrease in the price of tin, p_C, raises the probability that (12) will hold and has no effect on (13). If there are large asymmetries in the power of the two militia groups, then battles will be induced only by large charges in mineral prices. Asymmetry in military power helps bind the stationary-bandit equilibrium.[21]

[21] This result complements a large literature suggesting the probability of conflict in the absence of clear property rights – as opposed to cooperation - between opposing parties is higher when power is relatively symmetric (see, e.g., Umbeck 1981, Hirshleifer 1991, Skepardas 1991, Anderson and McChesney 1994, Fearon 1995, and Ralston

E. Effects of the Mining Policies

We hypothesize that Dodd Frank broke down a stationary bandit equilibrium. First, the policy sharply lowered the local price of minerals, particularly tin, tantalum and tungsten (see section 2). In our theory, a decrease in p_C makes looting agriculture relatively more attractive than taxing tin and thus raises the probability of looting (see equation 8). Second, the policies caused the local price of the 3Ts to fall more dramatically than the local price of gold (see section 2). In our theory, a decrease in the relative price, p_C / p_G, raises the probability of battle over gold mines in $t = 1$ (see equation 12). Third, the policies made the future of artisanal 3T mining more uncertain, plausibly shortening the planning horizon of the group controlling 3Ts. In our theory, a planning horizon of one-period rather than two periods guarantees looting.

Adapting our stylized theory to the real setting in the eastern DRC, of multiple armed groups and multiple mines, motivates the following testable hypotheses:

H1. The conflict mineral policies increased the probability of battles between armed groups near gold mining sites.

H2. The conflict mineral policies caused some armed groups that were stationed at 3T mines to abandon those mines and loot civilians, first near the 3T mining sites and then in surrounding non-mining, agricultural areas.

H3. The conflict mineral policies caused some armed groups that were stationed at gold mines to abandon those mines and loot civilians, first near the gold mining sites (in order to avoid battles with armed groups seeking gold territorial control), and then in surrounding non-mining, agricultural areas.

In addition, the theory hypothesizes that looting will concentrate in agricultural areas receiving favorable weather shocks, which is testable in the data.

4. Data for Empirical Analysis

To assess the theory, we employ data on armed civil conflict, the location of artisanal mining sites (described in section 2), and on world mineral prices.

2012). Similarly, the literature on the determinants of legal settlement versus litigation – which is a courtroom battle – contends that trials select from cases in which the two litigants have an equal chance of winning (see Priest and Klein 1984, Cooter and Rubinfeld 1989, Friedman and Wittman 2007).

A. *Conflict Data*

The conflict data come from the Armed Conflict Location and Event Dataset (ACLED).[22] This dataset reports information on internal conflict disaggregated by date, location, and by actor or actors for several unstable and African countries, including the DRC. The ACLED data currently covers 1997-2012 for the DRC, but our analysis focuses on 2004-2012. [23]

We begin the analysis in 2004, rather than earlier, to exclude the 1997 to 2003 period of the Second Congo War because our theory is silent on many of its complex determinants (see Stearns 2011). We end the analysis in 2012 rather than later, because we are interested in measuring the short-run impacts of the policies and because of current data availability.

The unit of analysis in ACLED is an event occurring on a specific date and at a specific location (longitude and latitude). The ACLED data are based on media reports and there is almost certainly measurement error in reported conflicts. As we explain later, the error would have to be biased in a very particular way both spatially and temporally to undermine our overall conclusions. Conflicts lasting multiple days are recorded as separate "atomic" incidents. This fact is important to consider when interpreting the data. The ACLED coding of events includes battles, violence against civilians, riots/protests, and non-violent events. We drop riots/protests from the analysis because our theory is silent on their determinants.

From the ACLED data we construct two dependent variables that coincide with outcomes in the theory. We construct the first, "looting", from verbal descriptions of each conflict.[24] The looting variable equals one if an armed militia group's actions are described by the words 'loot', 'pillage', 'plunder', 'rob', 'steal', 'ransack', 'sack', or 'seize.' There are 367 of 4218 non-protest/riot DRC events over 2004-2012 described with these words. Examples of "looting" events include: "Mayi Mayi Militia (DRC) attacked the mining quarry at Bibolo to loot", "FDLR attacked and looted the Mikweti village in the Walikale locality", "Unidentified armed group looted, pillaged and carried out rapes in Mukoberwa", "Mayi Mayi Militia (DR Congo) looted

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[22] The ACLED data are available at www.acleddata.com and are described in Raleigh et al. (2010).

[23] The ACLED data are employed in several economics and political science studies (see, e.g., Minoiu and Sehmyakina 2014) and we are aware of two other economic studies that employ DRC, ACLED data in empirical analysis. Maystadt et al. (2014) study the relationship between conflict and mineral prices during 1997-2007, and Pellillo (2011) uses the data to study the impact of conflicts on household assets. Maystadt et al. (2014) uncover a complex relationship between mining starts and violence that depends on the spatial scale considered. Pellillo (2011) finds negative effects of violence near villages on the accumulation of assets in villages.

[24] We use the ACLED dataset, rather than alternatives such as the UCDP, primarily because the ACLED dataset codes and includes looting events whereas the alternative data sets that we are aware of do not.

villages in the Beni area, taking food, goats etc." From the verbal descriptions and ACLED coding, it is clear that "looting" events are often violent, sometimes brutally so.

The second dependent variable, "battles" comes directly from ACLED coding. Battle events are between armed militia groups, including the Congolese army. Examples include "Mayi Mayi Militia clashed with FARDC in Kisele", and "FARDC launched an offensive against FDLR in Nduma." Forty-six percent of the ACLED DRC events are battles.

In addition to looting and battles, we employ two auxiliary dependent variables to help interpret the main findings and to examine possible channels. These are "violence against civilians" and "militia recruiting." Violence against civilians events come directly from ACLED coding and are perpetrated by militia groups. Based on text descriptions, violence against civilian events are often coupled with looting but are sometimes described as "attacks" on villages and may include "kidnappings" and "rapes." Forty-one percent of the 4218 ACLED DRC events are coded as violence against civilians. We construct our "militia recruiting" variable by flagging events described with the words "recruit", "enlist", or "draft." There are 64 events over 2004-2012 described with these words. Examples include: "Mayi Mayi Militia (Yakutumba) carry out a recruitment drive around Fizi"; "Reports that Former CNDP militia have attempted to recruit child soldiers in the area"; and "4 FARDC officers stationed at Goma begin a recruitment drive for young soldiers."

The ACLED data provide some information on the number of fatalities per event, but the true number of fatalities is often unknown or unreported, rendering the quantitative information unreliable for econometric analysis. For this reason, we analyze the number of conflict events and indicator variables for conflict rather than analyzing the number of fatalities.

Because the raw ACLED data are available in a highly disaggregated form, we must choose if and how to aggregate the data spatially and temporally prior to analysis. In all cases we are focusing on the Eastern DRC, rather than the full country. Spatially, our first choice is to pick between using administrative units (e.g., provinces, districts, territories, etc.) or imposing a spatial grid using GIS software and conducting analysis of cells within that grid. Out of these two options, we choose to analyze administrative units rather than cells of arbitrary size. Using administrative units seems preferable because these units presumably demarcate topographical, political, and cultural boundaries that could segregate conflict and to follow precedent in the empirical literature on DRC conflict (see, e.g., Maystadt et al 2014).

The administrative unit choices are the 5 provinces, the 12 districts, the 70 territories, or an unknown number of villages. Village level analysis is not feasible for our purposes because, to our knowledge, there is not a comprehensive mapping of villages in the DRC. Of the feasible options, we chose territory-level analysis because only it allows us to examine how conflict before and after Dodd Frank co-varied with mine density variation within North Kivu, South Kivu, and Maniema provinces which is critical for assessing our theory (see maps 3 and 4).[25] Conveniently, our rainfall data, which turn out to be an important determinant of conflict, are disaggregated into gridded units that are similar in size to the average sized territory as we discuss below. [26]

Temporally we focus our analysis at the monthly level, because our mineral price data can be observed at the monthly (but not daily or weekly) level, and because other researchers have also employed monthly observations (see Maystadt et al. 2014). Econometrically we employ monthly, territory-level conflict data over 2004-2012 which are summarized in Table 4.

B. Policy Indicator Variables

In our main analysis, we employ a single 'policy indicator' variable that assigns 'treatment' over time and across the territories most directly affected by the mining policies. Although the use of a single indicator variable forgoes some detail about the timing of different policies (e.g., the passage of Dodd Frank in July 2010, the mining ban in September 2010, the EITC boycott in April 2011), this simple choice has advantages. Importantly, it may be inappropriate to consider the policies following-up on Dodd Frank as separate and independent events when the passage of Dodd Frank almost certainly triggered the subsequent policies.[27] However, we also treat Dodd Frank and the ban as separate policies in some empirical estimates.

[25] We cannot examine how conflict varied with mine prevalence using province or district level analysis because province and district boundaries are the same for North Kivu, South Kivu, and Maniema.

[26] The mean size of territories in the eastern DRC is 17,912 km^2 (6,916 $miles^2$), which is similar in area to the smallest U.S. states or the largest U.S. counties. The area is slightly larger than the U.S. state of Hawaii and slightly smaller than New Jersey. The average territory area is about the same size as the average county size in the U.S. state of Nevada.

[27] Seay (2012) states: "Neither Kabilia's ban or the MSC's [EITC boycott] decision to stop buying Congolese minerals would have happened had Dodd-Frank not become law. Both the timing of the actual and *de facto* bans and all rhetoric surrounding them suggests that these were clear responses to the perceived future effects of the legislation. MSC and other international buyers are not purchasing Congolese minerals due to uncertainty about the SEC regulations on Section 1502."

We choose July 2010 as the time in which Dodd Frank 'treatment' began, although we recognize that formal regulatory authority of Section 1502 was not exercised until later. Our choice of July 2010 is less arbitrary than other times we could choose, and it is reasonable for two reasons. First, Section 1502 may have shortened planning horizons over mine control immediately, causing armed groups to react before specific regulations were written.[28] Second, observers argued that Dodd Frank was causing a de facto boycott of 3Ts shortly after it was passed, before the more official boycott began in April 2011 (Pöyhönen et. al 2010, Seay 2012).

For the spatial dimension of policy treatment, we designate a 'treated' group of territories to correspond with our conceptual landscape, which includes a gold mining region, a tin mining region, and several agricultural regions. We designate as 'treated' the union of the territories for which mining was banned (i.e., all territories in Maniema, North Kivu, and South Kivu) and the territories that the U.S. State Department identified as being exploited by armed groups as part of its role in complying with Section 1502. There are 27 'policy territories', which are illustrated as the shaded territories in maps 3 and 4. Seven of these territories had one or fewer mines and plausibly specialized in agriculture, rather than mining, but all territories had agricultural activity. Twenty policy territories had multiple mines. Seven territories specialized in gold mining, and five specialized in 3Ts. The remaining 43 'non-policy' territories comprise the control group of non-policy territories. A

Given our temporal and spatial designation of treatment, the policy indicator takes a non-zero value for 810 observations in our main regressions. (This is 27 territories x 30 months from July 2010-December 2012). The policy indicator variable equals 0.33 during July 2010, because Dodd Frank was not passed until July 21, 2010.

C. Mineral Prices

The mineral price data come from MetalPrices.com, which requires a subscription for historical data. From their website we have downloaded data on the world prices of gold, tin, tantalum, and tungsten. The gold prices are reported in dollars per troy ounce. The prices of 3Ts

[28] Section 1502 was added to Dodd-Frank late in the legislation process, in late May 2010, so a treatment date preceding July 2010 by more than a month or two would not be appropriate (see Woody 2012).

are reported in dollars per pound. Figure 5 shows the monthly averages for 2004 through 2012, which are CPI adjusted and reported in U.S. dollars. [29]

D. Rainfall Seasons and Shocks

In the theory, positive wage shocks in agriculture raise the incentives for armed groups to loot. To proxy exogenous shocks, we construct two sets of rainfall measures. First, we have followed Maystadt et al. (2014) by constructing a measure of territory-level rainfall anomalies. Like Maystadt et al., we assume that abnormally high – but not excessively high – quantities of rainfall act as positive shocks in the unirrigated agricultural regions of the eastern DRC. To construct the rainfall measure, we first downloaded precipitation data from Global Precipitation Climatology Center's (GPCC) website and then converted the monthly, spatially gridded data to territory-level averages using a process we describe in the appendix. Conveniently, the one-degree grids are similar in area to the average sized territory in the Eastern DRC.[30] Next, we calculated rainfall anomalies for each territory-month observation. Anomalies are the difference between rainfall during the specific month and the territory's 1951-2012 mean for that month. We divided the difference by the standard deviation in rainfall over 1951-2012 to standardize. The resulting variable has a mean of 0.063 and ranges from -2.66 to 3.59 (see Table 4).

To account for the possibility that rainfall seasons are also important determinants of conflict, we have constructed indicator variables for wet and dry season patterns within each territory. We identify the driest and wettest three months in each territory based on long run precipitation averages, from 1951-2012. The 'wet season indicator' equals one for territory *i* in a particular month if the long-run average precipitation for that month ranks among the highest three. Similarly, the 'dry season indicator' is equal to one for territory *i* in a particular month if the long-run average precipitation for that month ranks among the lowest three.

5. Empirical Analysis

The theoretical framework hypothesizes the mining policies triggered an increase in looting within the policy territories, both near mines and in the surrounding agricultural

[29] We lack detailed data on the time variation in prices received locally, which differ from world prices, especially after Dodd Frank according to on-the-ground anecdotes (see, e.g., Carish 2012).

[30] The mean size of territories in the eastern DRC is 17,912 km^2 (6,916 $miles^2$) and the gridded rainfall estimates are reported for grid cells of approximately 12,300 km^2.

territories. The framework also hypothesizes that the mining policies triggered battles between militia groups near gold mining sites. We provide statistical tests in this section, after first presenting graphical evidence.

A. Graphical Evidence

Figure 6 compares patterns of conflict inside versus outside the policy territories depicted in map 3. Focusing first on Panel A, we see that looting events in the 27 policy territories began to rise sharply at the end of 2010, after the passage of Dodd Frank, and stayed high through much of 2011 and 2012. As Panel B indicates, there was not a commensurate rise in looting in the 43 non-policy territories despite similar pre-Dodd Frank trends across the two regions.[31] In Panel D, we see that the number of battles increased in the policy territories after Dodd Frank was passed, at least relative to the number of battles in the non-policy territories post-Dodd Frank. The incidence of violence against civilians also increased in the policy regions after Dodd Frank and there was not a commensurate rise in the non-Policy regions (Panels E and F).

Figure 7 shows visual evidence of the location and timing of conflict events that is consistent with the theoretical predictions. The plots show conflicts per territory over a narrower time window, from 2008-2011, in order to give better perspective on the timing of conflict events. Panels C and D separately plot incidents of conflict in the mining and non-mining territories defined in map 4. Panel C indicates that mining and non-mining territories had similar levels of looting prior to Dodd Frank and looting was following similar - albeit erratic - trajectories during 2008 to July 2010 in both sets of territories. Looting increased after the passage of Dodd Frank in both sets of territories, and the increase in the mining territories preceded the increase in the non-mining territories. Panel E shows that looting increased in territories specializing in both gold and 3Ts. Panel F shows that battles were infrequent in both types of territories during mid-2009 until early 2011 when the number of battles increased sharply in the gold mining but not 3T mining territories.

B. Econometric Model

[31] If Dodd Frank policies caused spatial spillover of looting into the non-policy territories, then these comparisons are biased downward and understate the true effect of the policies. We address this issue in robustness checks.

To implement formal tests, we employ standard panel regression analysis. Our least restrictive model is of the form in (14), but we begin by estimating a baseline regression in which all coefficients except δ_i, μ_t, β_1, β_2, β_3 are zero. We sequentially relax the restrictions and allow the other coefficients to be non-zero.

$$
\begin{aligned}
(14)\qquad & conflict_{itk} = \delta_i + \mu_t + \omega_i t + \beta_1 policy_{it} + \beta_2(policy_{it} \times 3T_i) + \beta_3(policy_{it} \times gold_i) \\
& + \sum_{m=1}^{4} \lambda_m (mine_{i,m} \times price_{t,m}) + \sum_{k=1}^{2} \gamma_k season_i + \sum_{x=0}^{2} \eta_x rain_{i,t-x} + \sum_{x=1}^{3} \alpha_x conflict_{i,t-x} \\
& + \sum_{q=0}^{1} \phi_q adj.\ conflict_{i,t-q} + \varepsilon_{itk}
\end{aligned}
$$

Here *i*=the 70 territories, *t* = the 108 months spanning 2004-2012, and *k* = the season (dry, wet, or neither). The notation δ_i represents the 70 territory fixed effects and μ_t represents the 108 time effects. The notation $\omega_i t$ denotes individual linear time trends for each territory. The territory fixed effects help control for factors that are relatively time invariant, and known to be important determinants of conflict, such as ethnic composition, fractionalization, and geography (see Esteban et al. 2012). The time period effects control for eastern DRC-wide factors that may cause changes in conflict, such as presidential elections or changes in national inflation. The territory-specific time trends control for the possibility that conflict in a territory was already trending up or down prior to Dodd Frank.

The coefficient of interest are β_1, β_2, and β_3. The coefficient β_1 measures the effects of the policy in territories lacking mines, and β_2 and β_3 measure the effects of the policy interacted with the number of 3T and gold mines respectively (see map 3). Our theoretical framework predicts that $\beta_1 > 0$ in the looting regressions and that $\beta_3 > 0$ in the battle regressions.

The coefficients λ_m measure the relationships between conflict and monthly world mineral prices for *m* = tin, tungsten, tantalum, and gold, each interacted with indicators for the presence of each type of mine in a territory. The coefficients γ_k measure seasonal patterns in conflict with territory specific indicators (*k*= wet or dry seasons). The η_x coefficients measure the effects of contemporaneous and lagged rainfall anomalies on conflict.[32]

[32] Many papers have uncovered relationships between resource prices, climate shocks, and conflict (see Collier and Hoeffler 2004, Miguel et al. 2004, Miguel 2005, Angrist and Kugler 2008, Brückner and Ciccone 2010, Bohlken and Sergenti 2011, Hsiang et al. 2011, Dube and Vargas 2013, Maystadt et al. 2014, Sanchez de la Sierra 2014).

Finally, to check the robustness of the β coefficients to controls for the local persistence of conflict, we include lags for the number of conflicts in previous months in some specifications.[33] We also control for conflicts in adjacent territories, measured as the aggregate number of conflicts in all adjacent territories, to control for potential spatial spillover. In the context of regression equation (14), we allow α_x and ϕ_q to be non-zero. We include lags until they are no longer statistically significant in any specification that we have tried, which occurs at x = 3 month lags for lagged conflict and q = 1 month lags for adjacent territory conflict.[34]

Although the outcome variables represented by $conflicts_{itk}$ are binary and count data, we estimate the empirical model with OLS linear fixed effects estimators. We justify this choice on technical and practical grounds. On the technical side, we are reluctant to make assumptions about the distribution of the error terms in order to validate poisson or negative binomial estimators. On the practical side, we have found that our main inferences are not sensitive to estimator choice. In all estimates, we cluster standard errors at the territory level to account for possible serial correlation within territories (Bertrand et al. 2004).

C. Main Results

Table 5 provides the first set of estimates. The dependent variable in columns 1-6 is the looting indicator and the dependent variable in columns 7-12 is the battle indicator. The even numbered columns include territory specific time trends. Columns 1-2 and 7-8 are the baseline results with no additional covariates. Columns 3-4 and 9-10 add the mineral price and rainfall variables. Columns 5-6 and 11-12 introduce the lagged and adjacent conflict variables.

[33] These lags measure non-riots/protests conflict of all types in the ACLED data to allow for the possibility that a past battle may affect future looting and vice versa. The lagged conflict may be particularly important when the dependent variable is an ACLED monthly count of conflict episodes because extended conflicts are counted as separate incidents for each day they persist. This kind of structural autocorrelation is less of a problem when the dependent variable is an indicator variable for whether or not a conflict occurred during a given month.

[34] We introduce the dynamic and spatial lags mainly to test for robustness of the β coefficients, rather than due to inherent interest in the α and ϕ, but we recognize the potential biases that adding these variables create. The dynamic lags introduce the Nickell Bias (Nickell 1981). With positive serial correlation, our estimates of α understate the true persistence of conflict by a factor that decreases with T, the number of time periods. Because T is relatively large in our case, at 108, this bias is likely small. Including the adjacent territory variable potentially introduces a simultaneity or reflection problem (Anselin 2002). If conflict among neighbors is positively correlated, our estimates will overstate the true ϕ coefficients. Inclusion of the adjacent territory variable could also bias downward our estimates of the β coefficients if the policy indicator is positively correlated with adjacent conflict.

Turning first to the looting regressions in table 5, we see that the policy coefficient estimates, $\hat{\beta}_1$, are all positive, statistically significant, and fairly stable in magnitude across specifications. The coefficients on the interaction terms, $\hat{\beta}_2$ and $\hat{\beta}_3$, are not statistically different from zero. Hence, the evidence suggests that Dodd-Frank increased the probability of looting in the policy territories, and that this increase was not statistically different across mining and non-mining territories. This result is in line with our theoretical framework, which implies that looting would increase in 3T and gold regions and spill into agricultural regions. For perspective on the magnitudes of the estimates, the mean probability of looting during 2004 to July 2010 in the policy territories was 0.030. Hence, the column 5 estimate of 0.053 implies that the probability of looting in policy territories increased by 176 percent after Dodd Frank.

In the table 5 battle regressions, the $\hat{\beta}_1$ and $\hat{\beta}_2$ coefficients are not statistically distinct from zero but the $\hat{\beta}_3$ estimates are all positive and statistically significant. These coefficients indicate that battle probabilities did not rise in general within the policy territories, but battle probabilities did increase with the number of gold mines in a territory after the passage of Dodd Frank. This result is in line with our theoretical framework, which predicts that Dodd Frank would encourage battles over gold because the policy raised its value relative to 3T minerals. For perspective on the magnitudes of the $\hat{\beta}_3$ estimates, the mean probability of a battle prior to Dodd Frank in a policy territory with at least one gold mine was 0.126. The number of gold mines in the policy territories ranged from 0 to 69 with a mean of 15.7, conditional on having at least one gold mine. Hence, the column 10 coefficient of 0.003 means the probability of battle increased by 15.7 x 0.003 = 0.0471 in a policy territory with the mean number of gold mines. This is a 37 percent increase relative to the pre-Dodd Frank mean of 0.126.

Table 6 shows the same set of regression specifications but here the dependent variables measure the number of looting and battle incidents. In the looting regressions, $\hat{\beta}_1, \hat{\beta}_2$, and $\hat{\beta}_3$ follow the same pattern as those in table 5: the $\hat{\beta}_1$ coefficients are positive and significant and the $\hat{\beta}_2$ and $\hat{\beta}_3$ coefficients are not statistically different from zero. For perspective on magnitudes, the mean number of looting incidents in the policy territories was 0.047 prior to Dodd Frank. Hence, the column 6 coefficient of 0.152 means that looting increased by 323 percent. The battle coefficients of $\hat{\beta}_1$ and $\hat{\beta}_3$ in columns 7-12 follow the same general pattern as those in table 5 but are a less robust. The column 11 coefficient, for example, suggests that battles increased by 15.7 x 0.007 = 0.109 in the policy territory with the mean number of gold mines. This is a 29 percent increase relative to the pre-Dodd Frank mean of 0.374 in the territories with gold mines. Comparing the battle coefficients of $\hat{\beta}_3$ across table 5 and 6 indicates that Dodd Frank had a more systematic effect on battle probabilities in gold mining territories when compared to its effect on number of battle incidents.[35]

Turning to the coefficients on the covariates in table 5 and table 6, which are not our focus, we note the following patterns. First, the coefficients on the price-mine interaction terms, $\hat{\lambda}_m$, are generally insignificant and they are sensitive to the inclusion of time trends. This sensitivity may be due to the fact that mineral prices, especially gold, trended in systematic ways during much of 2004-2012 making it difficult to separately identify time trends from price effects. In any case, we are reluctant to draw conclusions from the price coefficients because the local prices received from the local sale of minerals reportedly deviated sharply from the world price during our period of study (see section 2).

Second, the rainfall variables are significantly related to conflict in some of the specifications. Looting episodes are more frequent during dry seasons (see table 6), and contemporary rainfall anomalies are positively related to the number of battle events (see table 6).[36] These results are arguably consistent with our theoretical framework. The rainfall result suggests that militia groups are more willing to engage in lengthy battles over prospective agricultural surpluses as the size of surpluses increase. Assuming the surpluses are realized in a time period after the favorable rain, the prize from the battle is actually for the right to loot a future harvest. This interpretation suggests that looting should be more likely and frequent in months following positive rainfall anomalies, which is what we find in the looting regressions in tables 5 and 6, which show positive coefficients on lagged rainfall anomalies. This interpretation

[35] The ACLED data count conflicts lasting multiple days as separate events (see section 4). Hence, the patterns of $\hat{\beta}_3$ coefficients in table 5 and 6 suggests that Dodd Frank increased the probability of battles but not their duration.

[36] The marginal effects of rainfall anomalies on conflict are declining as the magnitude of rainfall anomalies increase, as indicated by the negative sign on the rainfall anomaly squared variable. This is not surprising. Like Maystadt et al. (2014), we suspect that above average rainfall increases agricultural earnings in the eastern DRC but flooding does the opposite.

is compatible with the table 6 finding that looting episodes increase during a territory's dry season, after yields have been harvested and output is lootable. Other contributing factors may be that troops are more mobile during the dry season, and that the opportunity cost of looting is lower, because harvesting has already commenced.

To summarize the results in tables 5 and 6, Dodd-Frank appears to have caused a rise in the probability of looting, and in looting episodes, that was widespread across the targeted policy areas. Dodd Frank also appears to have caused a targeted rise in the probability of battles in gold mining territories. These results are consistent with our theoretical framework, which hypothesizes that the policies caused militia groups to loot civilians, and to battle for the relatively more valuable gold mining sites.

D. Robustness of Main Results

Table A1 and A2 in the appendix show a series of robustness checks that modify the sample or covariates relative to those reported in tables 5 and 6. Panel A drops urban territories from the analysis because of possible urban reporting bias in the ACLED data (see Eck 2012). Panel B interacts each of the four mineral prices with the number of mines rather than with indicators for the presence of a mine. Panel C includes the lags of the dependent variable on the right hand side rather than lags for the number of conflicts of all types. Panel D omits from the sample the 17 territories that are outside the policy region but directly adjacent to at least one policy territory. Omitting these policy-border neighbor territories helps control for the possibility that the difference and difference estimates are confounded by the spillover of conflict into the neighboring non-policy areas.[37] As tables A1 and A2 indicate, our main findings generally hold across these robustness checks.

Table A3 in the appendix also shows that the findings pass placebo tests that assign false policy passage dates, in July 2009 and July 2008, rather than the actual passage of Dodd Frank in July 2010. We also assign a January 2009 placebo date to correspond with the beginning of the Kimia II offensive, which was a coalition between competing armed groups in the eastern DRC (see Sanchez de la Sierra 2014). Of the 36 placebo coefficients for looting, none are significantly

[37] This technique of omitting spatial units that are adjacent to treated spatial units is commonly used in difference and difference analyses of natural resource booms and busts (see Black et al. 2005, Michaels 2011, Jacobsen and Parker, forthcoming).

correlated with looting probabilities or looting incidents. These null findings further substantiate the table 5 and 6 evidence that Dodd Frank increased looting in the average policy territory. Of the 36 placebo coefficients for battles, only the July 2008 coefficient for $\hat{\beta}_1$ is correlated with the probability of battle (see table A3, column 12, row 1). We do not think this finding undermines the strength of our table 5 findings for two reasons. First, the key table 5 findings are the positive estimates of $\hat{\beta}_3$ rather than $\hat{\beta}_1$. Second, the $\hat{\beta}_1$ estimates for the January 2009 and July 2009 placebo tests are insignificant, suggesting the rise in battles is attributable to a surge during July 2008 to January 2009, well before the passage of Dodd Frank in July 2010.

E. Separate and Aggregate Effects of Dodd Frank and the Mining Ban

Table 7 presents estimates from a version of the model that allows the effects of Dodd Frank to differ from the effect of the mining ban. The econometric model is given below. Our primary interest is in estimating υ_1 and υ_2. To simplify interpretation and to give the model adequate statistical power, here we do not interact the policy variables with the number of 3T and gold mines.

$$\begin{aligned} conflict_{itk} &= \delta_i + \mu_t + \omega_i t + \upsilon_1 Policy_{it} + \upsilon_2 Ban_{it} + \sum_{m=1}^{4} \lambda_m (mine_{i,m} \times price_{t,m}) \\ &+ \sum_{k=1}^{2} \gamma_k season_i + \sum_{x=0}^{2} \eta_x rain_{i,t-x} + \sum_{x=1}^{3} \alpha_x conflict_{i,t-x} + \sum_{q=0}^{1} \phi_q adj.\ conflict_{i,t-q} + \varepsilon_{itk} \end{aligned} \tag{15}$$

Here υ_1 is the average effect of the policy indicator, which includes the mining ban. Hence υ_1 is the policy effect outside of the mining ban period (mainly after March 2011) and for targeted territories outside of Maniema, North Kivu, and South Kivu. The coefficient υ_2 measures any additional effect of the mining ban. For a territory under the mining ban (during September 2010 to March 2011), the full policy effect is $\upsilon_1 + \upsilon_2$.

Estimates of (15) provide tests of the theoretical model because Dodd Frank and the mining ban had different effects on the incentives of armed groups to engage in looting or battles. The key difference is that the mining ban declared artisanal mining illegal, whether gold or 3Ts, although the ban's applicability to gold was uncertain. Assuming symmetric effects on the net value of taxing gold and 3T mining sites, we expect the ban to have increased incentives to loot, but not to battle for gold because the ban did not raise the relative value of gold. Hence, our theory implies $\upsilon_1 + \upsilon_2 > 0$ in the looting regressions and $\upsilon_1 + \upsilon_2 = 0$ in the battle regressions.[38] Dodd Frank, by contrast, was a boycott on the traceable 3Ts rather than the difficult-to-trace gold. We therefore expect Dodd Frank to have increased incentives to loot and to battle for gold territories. Hence, our theory suggests $\upsilon_1 > 0$ in both the looting and battle regressions.

Table 7 shows the results. In the panel A regressions, the dependent variables are the conflict indicators and in the panel B regressions the dependent variables measure the number of incidents. The sequence of covariates is identical to the sequence in tables 5 and 6. Consider first the looting regressions in columns 1-6. The policy coefficients, $\hat{\upsilon}_1$, are all positive and are statistically significant. By contrast, the mining ban coefficients, $\hat{\upsilon}_2$, are effective zero implying $\hat{\upsilon}_1 + \hat{\upsilon}_2 > 0$. This result suggests that the policy-induced reduction in mining opportunities for both gold and 3Ts led to more looting in across the areas targeted by the policies. In the battle regression of columns 7-12, the $\hat{\upsilon}_1$ coefficients are positive and the $\hat{\upsilon}_2$ coefficients are negative. F-tests of all battle coefficients (not shown in table 7) fail to reject a null hypothesis of $\hat{\upsilon}_1 + \hat{\upsilon}_2 = 0$ at any *P* value less than 0.19. Hence, these findings suggest that Dodd Frank, rather than the mining ban, led to more battles because of its asymmetric regulation of gold and 3Ts.

As we noted in section 4, it may be inappropriate to treat Dodd Frank and the mining ban as separate independent policies because the mining ban was prompted by the passage of Dodd Frank. To understand the full aggregate effects of Dodd Frank, we estimate a version of (15) that forces $\upsilon_2 = 0$ by removing the mining ban indicator from the estimating equation. Table 8 shows the results. Not surprising, given the findings already presented in tables 5-7, the policy coefficients are positive across all of the looting specifications (columns 1-6). This is restatement of our earlier punchlines: Dodd Frank appears to have caused an increase in looting across the policy region ranging from a 156 to 240 percent increase in the probability of looting and a 294 to 391 percent in the number of looting incidents. The coefficients are less robust in the battle regressions, meaning there is only weak evidence that Dodd Frank increased battle probabilities and incidents in aggregate. An interpretation of all of the results in table 5-8 is that Dodd Frank increased overall looting, and it displaced battles into gold territories after the ban was lifted.

[38] Our theory assumes a stationary bandit starting point but if we relaxed this assumption and assumed ongoing battles for mineral control from the onset than we might expect $\upsilon_1 + \upsilon_2 < 0$ in the battle regressions.

Because looting and battles could both cause violence against civilians, we conclude by testing for the policy effects on violence against civilian which are directly coded by ACLED (see section 4). Table 9 presents the results for estimates of the β coefficients in (14) and for the υ coefficients in (15). In general, the pattern of estimates mimics the patterns in the looting regressions. The policy indicator is positively associated with violence against civilians, and the policy effect is not robustly different in gold mining territories or during the mining ban period. In terms of magnitudes, the range of point estimates of 0.054 to 0.156 in columns 1-6 of panel C are relative to a pre-Dodd Frank mean of 0.118. Hence, the probability of violence against civilian event increased from 46 to 132 percent. The columns 7-12 point estimates of 0.291 to 0.720 are relative to pre-Dodd Frank mean of 0.203, implying a range of increases between 143 and 354 percent. These findings suggest that Dodd Frank caused increases in violence against civilians, primarily because it generated incentives for militia groups to loot.

F. Limitations and Connection to Related Empirical Findings

Two limitations to our findings are worth emphasizing. First, there is almost certainly measurement error in the ACLED conflict data. Such errors would have to be biased in a particular way - i.e., improved measurement of battles in gold territories after Dodd Frank and improved measurement of looting in policy regions after Dodd Frank – to undermine our overall conclusions. In general, we would be most concerned about biased measurement error if we thought ACLED data underreported conflict during 2004 to July 2010, but fully reported conflicts after Dodd Frank. However, conflict data collected by other researchers working in the eastern DRC suggests the pattern of ACLED measurement error is actually biased against our finding of greater conflict after Dodd Frank.[39] Second, the composition of the control and treatment areas – i.e., the policy and non-policy areas – unlikely remained the same because miners likely relocated across territories for income opportunities. We cannot directly control for this factor or assess its importance because we lack territory-level data on populations. If the policies shifted populations away from 3T areas towards gold and agricultural areas, as we expect, then our coefficients underestimate the per-capita increases in conflict in 3T territories.

[39] Sanchez de la Sierra (2014) compares ACLED conflict data, aggregated to the annual level, with villager recollections of conflict in the eastern DRC. For the sample of villages surveyed, the ACLED data track villager recollections very closely for 2008 and 2009. For 2010-2011, however, the ACLED data underreport conflicts relative to villager recollections (see Figure 20).

Because of the empirical limitations just described, it is useful to emphasize how our findings are complemented by qualitative information on the effects of Dodd Frank and the mining ban. One example comes from Bisie, which was the largest artisanal tin mine in North Kivu. Prior to the mining ban, Bisie was home to about 13,000 people including 3,000 miners who were able to keep about 50 percent of the cassiterite mined and were taxed for the remainder by militia groups. Shortly after the imposition of the ban, 1,200 miners left for the nearby town of Ndjingala, and most remaining miners left Bisie by October 2010. According to Wimmer and Hilgert (2011, 8) roving armed groups routinely robbed and physically harassed former miners en route from Bisie and surrounding mines to the trading town of Ndjingala.

Geenan (2012) researched South Kivu mining villages before and during the mining ban. She concludes the ban led to greater incidences of thefts, robberies, armed attacks, and murders. Other assessments describe rising militia violence to take control of gold mining sites in the Kivus and Maniema after the conflict mineral policies (see United Nations 2011, Carisch 2012).

Our empirical findings also complement those of Maystadt et al. (2014) and Sanchez de la Sierra (2014). Maystadt et al. (2014) find a robust relationship between new mining concessions in the DRC during 1997-2007 as we discuss below in section 6. Our findings are also generally consistent with Sanchez de la Sierra (2014), who finds that armed groups violently established themselves at coltan mining villages, and asserted taxes, following a sharp rise in coltan prices during the early 2000s. He examines the decision by armed groups to become stationary or roving bandits, but, in his study the decision is motivated by price shocks and physical characteristics of minerals rather than the policy shocks we examine.

6. Alternative Interpretations of Empirical Patterns

To summarize the results in tables 5-9, Dodd-Frank appears to have caused a general rise in looting and violence against civilians that was not specific to territories dominated by mining prior to Dodd Frank, and a targeted rise in battles over gold mining territories. These results are consistent with our theoretical framework which hypothesizes that the policies caused militia groups to loot and rove, and to battle for the relatively more valuable gold mining sites. Some of the results are also consistent with alternative theories, especially those emphasizing the effect that a mining boycott could have on the opportunity cost of militia participation and on the displacement of crime from 3T territories. We discuss these and other issues in turn.

A. Reallocation to Unobserved Mining Sites

One possible explanation for our findings is that armed groups reallocated aggression towards 3T mining sites absent in the IPIS data. Although we cannot rule out this mechanism, it strikes us an unlikely driver of our main results. To explain our finding that looting rose in all policy territories, any reallocation of aggression to unobserved 3T mining sites would have to be evenly distributed across the policy territories. To our knowledge, the distribution of 3T mineral endowments in the eastern DRC is not even across territories. To explain our finding that battles increased disproportionally with the number of observed gold mines in a territory, any reallocation of aggression to unobserved 3T mining sites would require the unobserved sites to be concentrated in territories with a large number of observed gold mining sites. To our knowledge, the distribution of 3T endowments in the eastern DRC does not closely overlap with gold endowments.

B. Power Endowments, Information Asymmetries, and Commitment

Our theoretical framework considers the relative size of militia groups as a determinant of battle probabilities but we do not model the role of information asymmetries or commitment problems. These "rationalist" explanations for war could affect the decision to wage battle in general (see Fearon 1995) and potentially in our setting. In terms of commitment problems, Dodd Frank may have made peace alliances between militia groups more tenuous due to uncertainty over the future value of territory control and uncertainty over the future strength of various militia groups. This could have in principle triggered battles if the competing militia groups could not formulate incentive compatible cease fire agreements or alliances in the less certain environment. The Dodd Frank policy presumably also changed the relative military strength of groups and created new information asymmetries about that strength. This is possible, for example, if one militia had better access to gold than another or if there was asymmetric information about the extent to which the groups had access to new revenue sources. We cannot rule out these channels and we speculate that each played a role in triggering battles. These channels would not necessarily concentrate battles disproportionately in gold mining territories, however. It is also not clear how these channels would affect looting, which is not an outcome of focus in rationalist theories of war.

C. *Opportunity Cost of Militia Participation*

By holding constant the size of militia groups, our theoretical framework focuses on how the policies affected the spatial allocation of a fixed amount of armed soldiers. Our framework does not allow for soldier entry or exit, which we cannot directly observe in our data sets. By reducing mining opportunities and local prices for 3Ts, however, the policies presumably lowered the opportunity cost of militia participation and could have augmented militia sizes.[40] This mechanism could be a contributing factor to the increase in looting and violence against civilians that we observe in tables 5-9. Such a mechanism would be consistent with a large literature suggesting that negative shocks to resource values increases the opportunity cost of fighting, drawing labor into conflict activities (see Becker 1968, Grossman 1999, Chassangy and Miquel 2009). It would also be consistent with some commentators in the eastern DRC who worried that Dodd Frank could cause young men to join militias (Pöyhönen et. al. 2010).

The argument that Dodd Frank would lead to larger militias is inconsistent, however, with the assumptions of the policy makers who sought to reduce the size and strength of militia groups by cutting off revenue sources. With reduced revenues, the militia groups would have difficulty supporting new entrants with supplies, weaponry, and food and this would presumably reduce incentives to join militias (and could cause exit).

We cannot observe enrollment in militia groups, but we attempt to shed light on the effects of Dodd Frank on militia entry by analyzing the "recruitment" indicator variable described in section 4. This variable is equal to one if an event is described by the words "recruit", "enlist", or "draft." Table 10 presents regression estimates of the recruitment variable that follow the same specification sequence as the previous regression table 9. Columns 1-6 employs the 2004-2012 sample that we have used in all previous regressions. Columns 7-12 report results for a sample that is cut off at the end of 2011. Cutting the sample after 2011 is useful for two reasons. First, it focuses our attention on a time period in which armed group weaponry and other resources that complement soldiers had probably not yet been significantly diminished by Dodd Frank. Second, it also focuses attention on a time period before unemployed miners may have relocated from one territory to another for income opportunities.

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[40] Moreover, if the policy caused more entry into agriculture or other industries, and the wage in those industries fell as a result, then the policies could have further incentivized militia entry through the opportunity cost channel.

Two table 10 sets of results from the 2004-2011 sample arguably imply that recruiting episodes became more likely when and where the opportunity cost of entry was driven down by the mining policies. First, the panel A coefficients on the interaction between the policy indicator and the number of 3T mines is positive and bordering on statistical significance with *P* values ranging from 0.109 to 0.228. This result provides weak evidence that recruiting increased in territories most exposed to the boycott and ban. Second, the positive panel B coefficients on the mining ban and the negative coefficients on the policy indicator indicate that recruiting episodes were more likely during the mining ban but less likely in the 2011 months that followed.[41] These results are linked to opportunity cost because the all-out ban on artisanal mining presumably lowered the opportunity cost of militia entry to a greater extent than the Dodd Frank boycott that did not extend to gold.

In summary, our investigation of recruitment provides weak evidence that the mining policies had a short-run positive impact on recruitment (and possibly entry). The spatial pattern of recruitment does not match the pattern of looting, battles, or violence against civilians across the policy territories, however. Whereas recruitment increases were disproportionally confined to 3T areas during the mining ban, the increase in looting was widespread and the increase in battles was disproportionally concentrated in gold territories. We conclude from this coarse analysis that the policy-driven decline in opportunity cost may have contributed to militia strength, but it does not explain where the conflict occurred.

D. Crime Displacement

Theories in the literature on crime displacement are closely related to our stylized 'bandits' theory and potentially explain some patterns of increased conflict that we observe in tables 5-9. Crime displacement refers to the indirect effects of police interventions, or related policies against crime, that are focused on a particular illicit industry or on a particular neighborhood. Displacement occurs when a reduction of crime in the targeted area is offset by an increase in crime in other industries or neighborhoods. Displacement is caused by the behavioral responses of criminals to changes in the relative net return of criminal activity. For example, a police crackdown on crime in one neighborhood will raise the net return of committing crime in

[41] The claim that recruiting increased during the mining ban is supported by weak evidence because *F* tests of the panel B coefficients in columns 7-12 reject the null hypothesis of $\hat{\upsilon}_1 + \hat{\upsilon}_2 = 0$ for *P* value ranging from 0.11 to 0.22.

an adjacent neighborhood and rational criminals may relocate their activities. More generally, displacement has been referred to as the foreclosure of one type of criminal activity shifting the incidence of crime to different forms, times, and locales (Repetto 1976).

According to Draca et al. (2010), the issue of crime displacement has been considered much more in criminology than in economics[42] and our observation is that crime displacement reasoning has only sparsely been used to explain civil conflict over natural resources in the economics literature. An exception is Maystadt et al. (2014), who use crime displacement reasoning to explain empirical relationships between new mining concessions in the DRC during 1997-2007 and violent conflict. They find that conflict occurred at the periphery of mining sites rather than near the mines themselves. They theorize that this empirical pattern occurred because armed groups sought to ensure that mineral production was not disturbed by nearby violence, intentionally displacing violence away from economically productive areas.

The relevance of crime displacement to our findings depends on whether or not taxing, looting, and battles are aptly analyzed as criminal events, and whether or not the Dodd Frank boycott is properly conceived as a crime enforcement effort or crackdown. If one accepts the crime terminology as descriptive of our setting, then our findings have the following interpretation: the mining policies caused criminals (armed militia groups) to substitute one type of criminal activity (concentrated taxation of miners) with another (dispersed looting of villages) and geographically displaced another crime (battles) to other industries and territories (gold).

This generic characterization is reasonable but not fully descriptive of the setting and consequences of Dodd Frank, in part because the policy was targeted boycott rather than an enforcement crackdown. Moreover, as we explain in section 2, militia taxing of miners was often regular, predictable, and consensual whereas looting was not. In return for paying taxes, some miners received protection and sometimes miners directly solicited an armed group presence for that purpose.[43] Empirically, only six DRC conflict events are described in ACLED with “tax” whereas “loot”, “ransack”, “steal”, etc. is a common description. In other words, taxing at mining sites resembled a legitimate and productivity enhancing governmental function whereas looting

[42] Presentations of the issues in the criminology literature include Repetto (1976), Barr and Pease (1990), Guerette and Bowers (2009), Braga (2001), Hesseling (1994), and Sherman and Weisburd (1995). A recent crime displacement analysis in economics is Dell (2014), who studies crackdowns on Mexican drug trafficking.
[43] Sanchez de la Sierra (2014) provides several examples of this process. In addition, Carisch (2012, pp. 27) describes a situation in which an armed group (the FRF) received logistical and general support from mining communities in return for protection. The FRF raised revenues from taxing local markets and gold traders.

resembles the actions of common criminals. Hence, our findings imply the Dodd Frank boycott led groups to abandon legitimate government functions in favor of common and violent criminal activity. Although it is possible to view this chain of events through the lens of crime displacement, we think that Olson's (1993) stationary and roving bandits framework is somewhat more enlightening.

7. Conclusions

The top-down decisions to ban artisanal mining in three eastern DRC provinces and to regulate manufacturing and processing companies using those minerals did not reduce conflict during the time period we study. The long term effects remain to be seen, but the short term effects appear to have been devastating for some Congolese civilians. Instead of reducing violence, our findings suggest the policies have increased the likelihood and the number of episodes in which armed groups in the eastern DRC looted civilians and committed violence against them.

Our results and approach contribute to the literature on the interaction between natural resource prices, endowments, and violence. We join others in concluding that the resource curse, as it pertains to violent conflict, is a complex phenomenon that is unlikely solved by international trade embargoes or boycotts that reduce the value of a country's natural resource endowment.[44] Moreover, our findings call into question the widespread use of new forms of resource governance interventions that rely on strong assumptions about how natural resources are linked to conflict motivations (see Cuvelier et al. 2014). Our findings are a cautionary tale about what can go wrong if an international intervention cuts off revenue from some natural resources without fully anticipating the dynamic effect that the intervention will have on the incentives of armed groups and their preferred interactions with civilians.

Methodologically, our findings highlight the importance of within-country analysis in detecting policy-induced regional shifts in violence that are difficult to identify with country-wide data (see, e.g., Dell 2014). In our case, the resource-value shock induced by Dodd Frank does not appear to have increased the probability and incidence of battles in aggregate across the

[44] Several recent studies cast doubt on the generalization that resource endowments are the cause of violence in conflict ridden countries (see, e.g., Brunnschweiler and Bulte 2009).

targeted territories, but it apparently did shift battles from territories endowed with regulated minerals to territories endowed with unregulated gold.

We also offer a theoretical premise – that of stationary and roving bandits - from which to analyze armed civil conflict that we view as complementing rapacity, opportunity cost, and crime displacement models. Under the bandit premise, civilians pay taxes to warlords in exchange for crude protection. This informal institutional arrangement is not first best, of course, but it may be safer and more economically productive than anarchy (see Hirshleifer 1995, Skaperdas 2001, Olson 1993). Policy-makers in rich nations should be aware of this possibility when considering whether or not to intervene in ways that affect informal property rights and warlord incentives in foreign land.

Finally, we have focused on the short-run impacts of Dodd Frank but an examination of the longer run impacts could be fruitful. As other economists have suggested, reducing revenue from mineral trade to illicit militias could have the important effect of reducing their stock of weaponry (Janus 2011, Collier and Hoeffler 2004), which advocacy groups claim has already happened. A reduction in militia weaponry will not necessarily reduce violence in the longer run, however. The reduction could actually raise the probability of battles between militia groups if, for example, the decrease in weaponry led to more symmetry in the strength of competing militia groups and hence greater willingness to fight (see Ralston 2012). These important possibilities are beyond the scope of our paper, but we hope they are addressed by future research.

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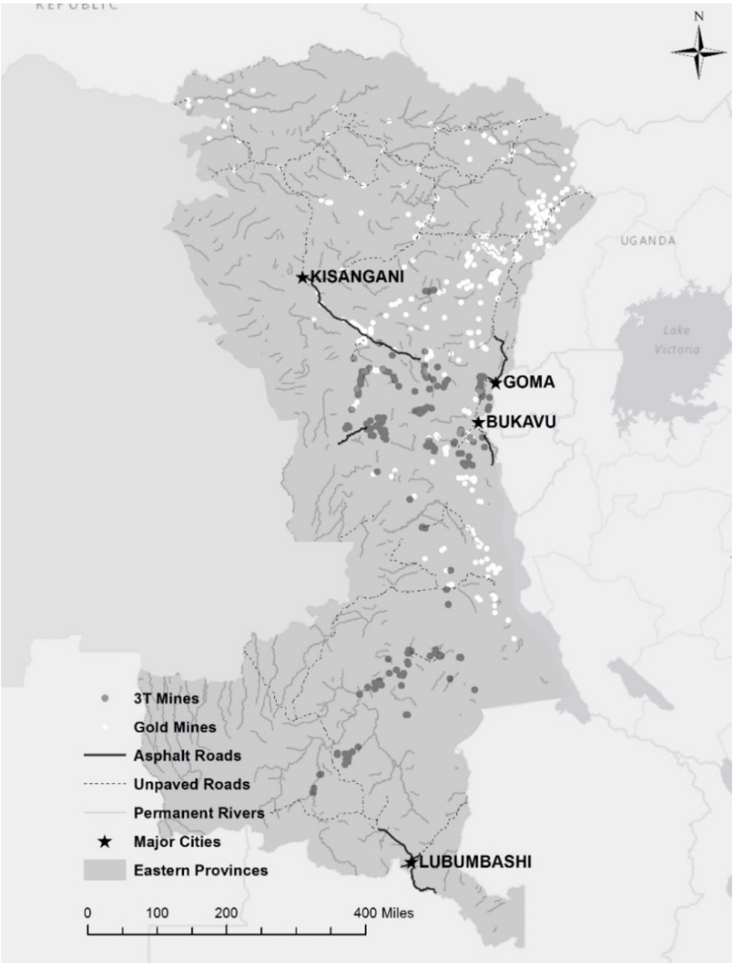
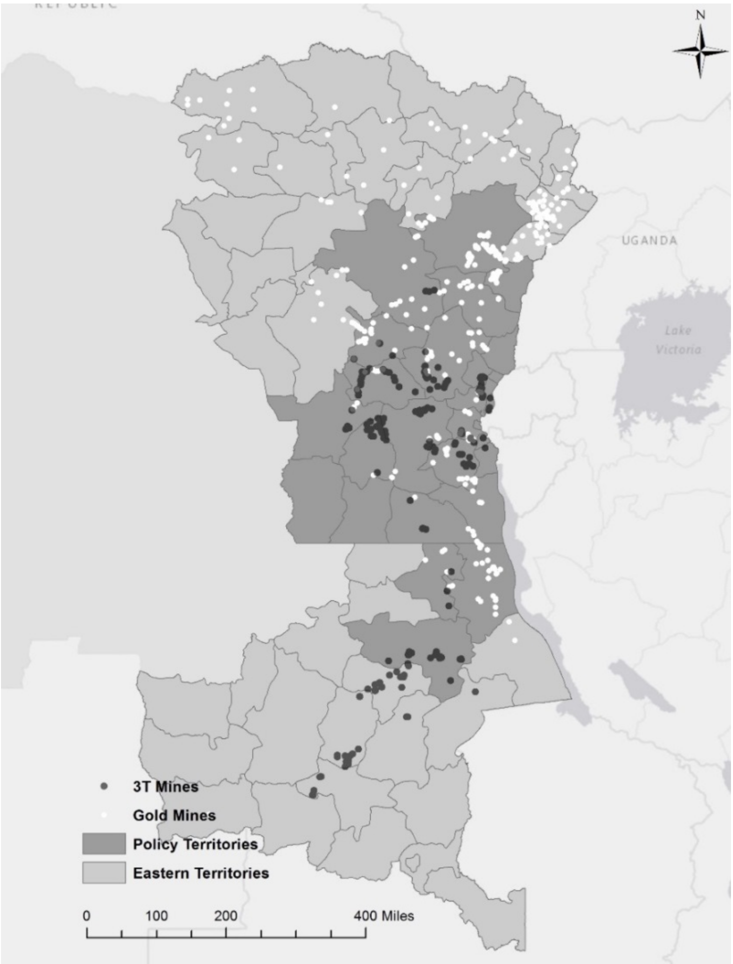
Map 1: The Five Eastern Provinces of the DRC

Map 2: IPIS Mines and Infrastructure in the Eastern DRC

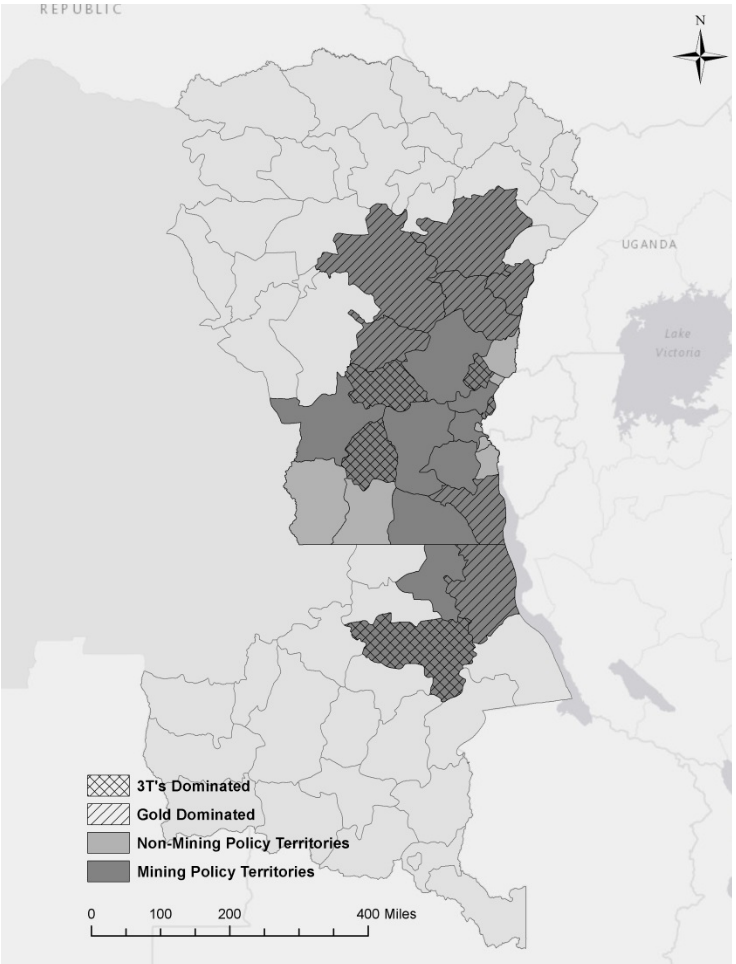


Notes: The boundary shapefiles as well as data on roads, rivers and major cities come from the USAID GIST Data Repository, available at: https://gistdata.itos.uga.edu/. The data on mine locations cine from a series of International Peace Information Service (IPIS) interactive maps described in detail by Spitaels and Hilgert (2008), Spittaels and Hilgert (2009), Spittaels (2010), and Spittaels and Hilgert (2010). The mines are listed by primary mineral but, in some cases, more than one mineral is mined from a site.

Map 3: Conflict Mineral Policy Territories and Mines



Map 4: Categories of Policy Territories



Notes: The "Policy Territories" comprise the union of territories in which mining was banned (i.e., all territories in Maniema, North Kivu, and South Kivu) and the territories that appear on the U.S. State Department's map of conflict zones. That map was commissioned in compliance with Section 1502 to identify areas and mines controlled by armed groups. It has been declassified and is available at: https://hiu.state.gov/Products/DRC_MineralExploitation_2011June14_HIU_U357.pdf. The map 4 categorizations are as follows. The seven non-mining policy territories had one or fewer IPIS mines. The 20 mining policy territories had more than one mine, ranging from a minimum 6 mines to a maximum of 69. A territory is "gold dominated" if the number of gold mines exceeded the mean number of 10.44, and if the number of gold mines is more than triple the number of 3Ts mines. A territory is "3T dominated" if the number of 3Ts mines exceeded the mean number of 7.44, and if the number of 3T mines is more than triple the number of gold mines. There are 7 gold dominated territories and 5 3Ts dominated territories by these definitions.

Figure 1: Timeline of Key Regulations

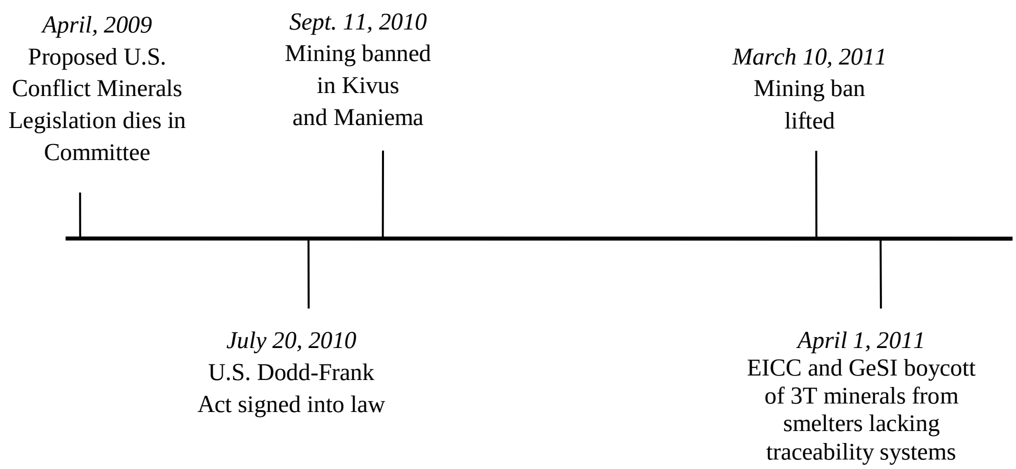


Figure 2: Official Exports of Tin and Estimated Production of Gold

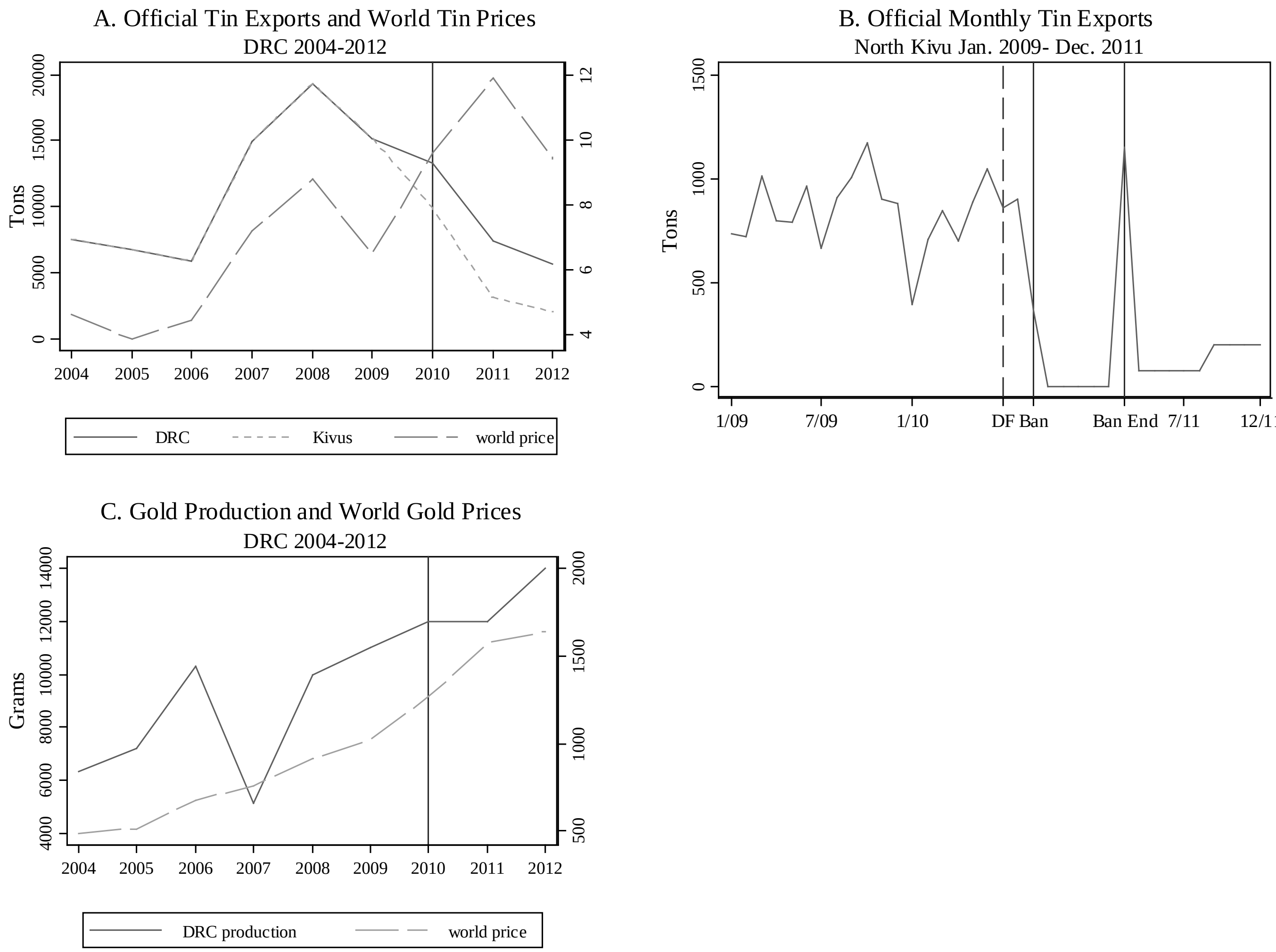


Figure 3
Decision Tree for Group 1 with a One-Period Planning Horizon

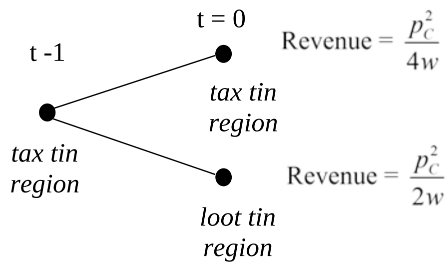


Figure 4
Decision Tree for Group 1 with a Two Period Planning Horizon

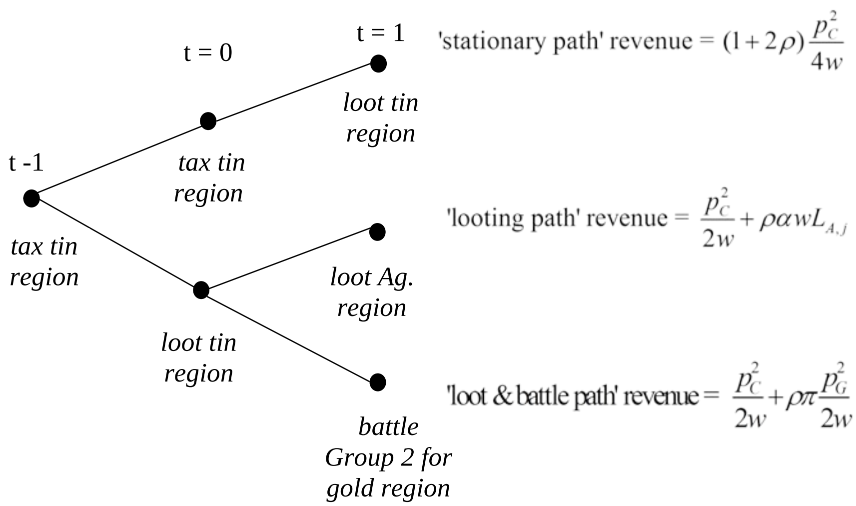
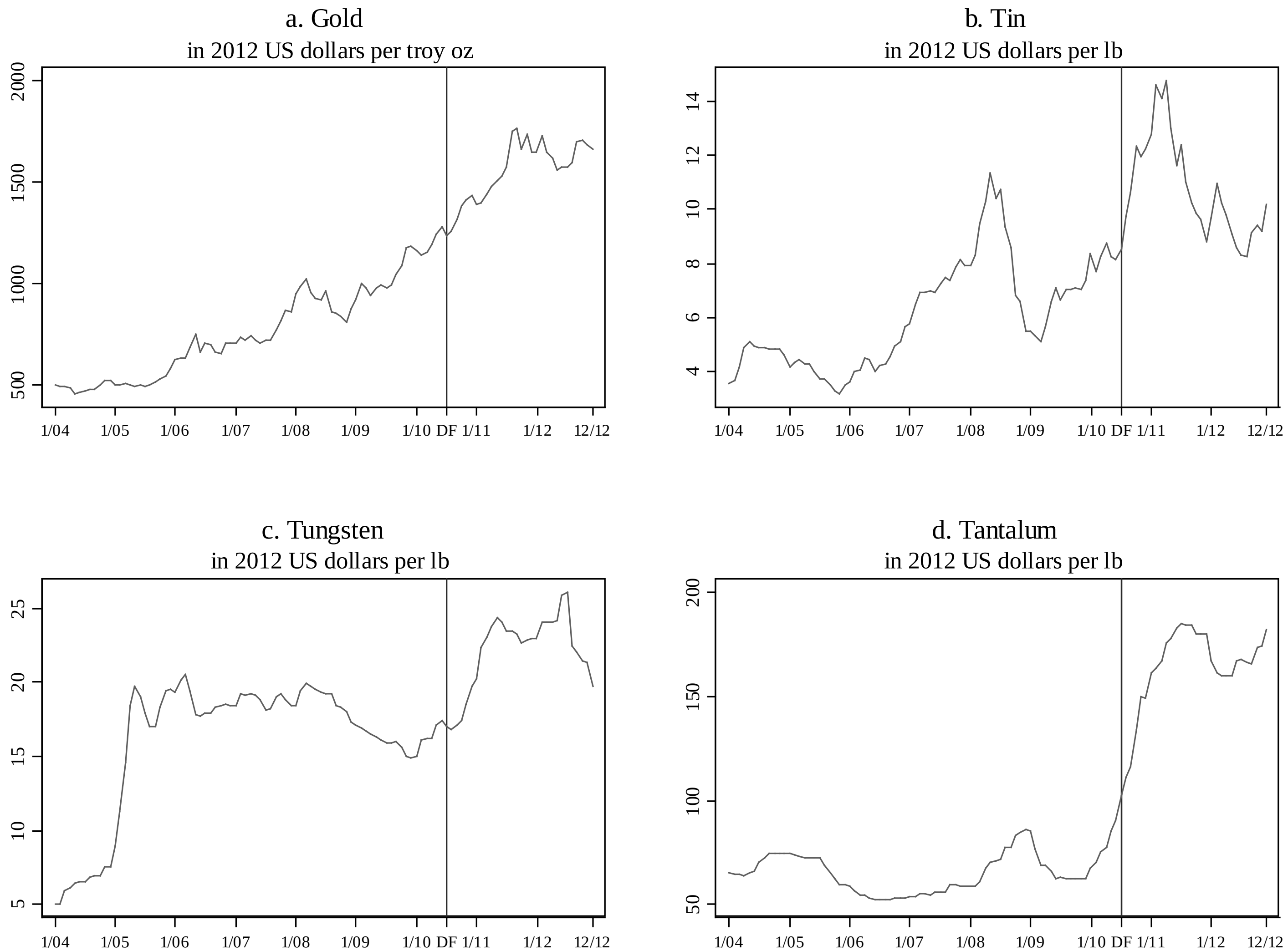
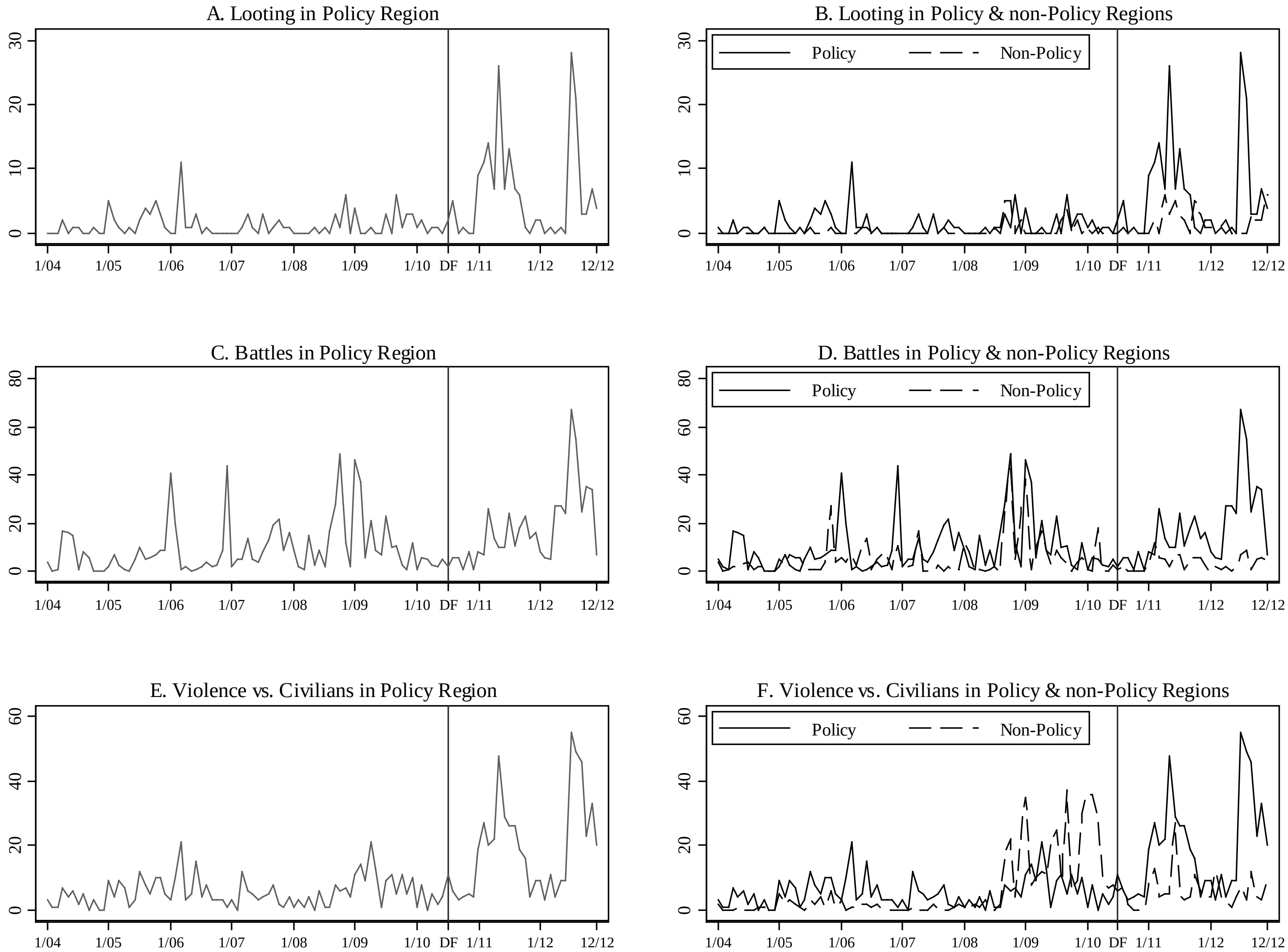


Figure 5: Monthly World Prices of Conflict Minerals, 2004-2012



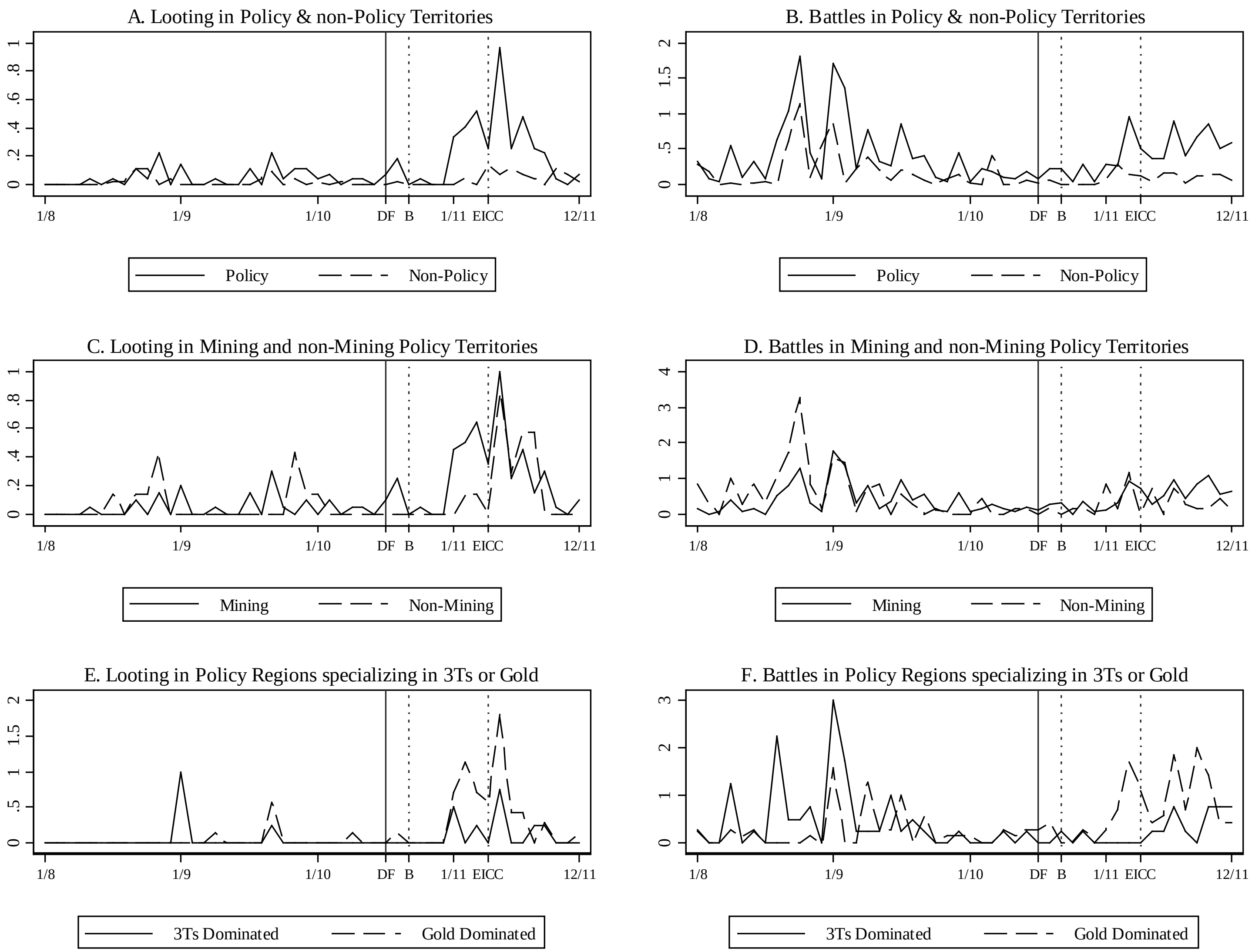
Notes: The source is MetalPrices.com, online subscription. The gold price represents the monthly average of the PM spot prices on the London Market Exchange. The tin price is the monthly average of the cash official price paid by buyers on the London Market Exchange. The tungsten price represents the monthly average of the ferro tungsten alloy price. The tantalum price represents the monthly average price paid for tantalum scrap by U.S. Vacuum Processors. "DF" indicates the passage of Dodd-Frank.

Figure 6: Monthly Conflict Incidents in the Eastern DRC, 2004-2012



Notes: The source is the ACLED database (Raleigh et al. 2010). The policy regions and outcome variables are defined in section 4. "DF" indicates the passage of Dodd-Frank.

Figure 7: Per Territory Means of Monthly Conflict Incidents in the Eastern DRC, 2008-2011



Notes: The source is ACLED (Raleigh et al. 2010). The policy regions and outcome variables are defined in section 4. "DF" indicates the passage of Dodd-Frank, "B" indicates the mining ban, and EICC indicates the boycott on tin, tungsten, and tantalum from smelters lacking traceability systems. Map 4 shows the gold and 3Ts dominated territories.

Table 1: Mining Sites in Eastern DRC Prior to Conflict Mineral Policies

(by primary mineral)

	Mines	Gold	Cassiterite	Coltan	Wolframite
Source: MiMiKi Map					
N. Kivu	96	67	19	9	1
S. Kivu	118	58	52	2	6
Source: Hinterlands Map					
Katanga	56	20	31	5	0
Maniema	144	59	80	4	1
Orientale	85	82	2	1	0
Source: Orientale Map					
Orientale non-Hinterlands	116	116	0	0	0
Source: Katanga Update Map					
Katanga non-hinterlands	44	13	29	2	0
Total	*659*	*415*	*213*	*23*	*8*

Notes: The data are listed by primary mineral but, in some cases, more than one mineral is mined from a site. The hinterlands map inventoried mines from a subset of territories in Katanga, Maniema, and Orientale. For Katanga, the territories covered are Kalemie, Malemba-Nkulu, Manono, and Nyunzu. For Maniema, the territories covered are Kabambare, Kailo, Lubutu, Pangi and Punia. For Orientale, the territories covered are Bafwasende and Mambasa. The Orientale map covers the entire Orientale province, but with less detail than the Orientale Hinterlands map. The maps are described in detail by Spitaels and Hilgert (2008), Spittaels and Hilgert (2009), Spittaels (2010), and Spittaels and Hilgert (2010).

Table 2: Militarized Mining Sites in Eastern DRC Prior to Conflict Mineral Policies

(by primary mineral)

Armed Group Present	**Mines**	**Gold**	**Cassiterite**	**Coltan**	**Wolframite**	**Known workers**
None Identified	314	150	144	10	7	60,252
FARDC	142	100	31	9	1	42,493
FDLR	37	24	12	1	0	4,801
Mayi-Mayi Militias	16	8	6	1	0	6,125
PNC	11	0	11	0	0	15,899
FRF	7	7	0	0	0	---
Other	1	0	1	0	0	400
Total	*528*	*289*	*204*	*21*	*8*	*129,570*

Notes: The data come from the MiMiKi and Kivu Hinterlands maps described in Spittaels and Hilgert (2009) and Spittaels (2010). The MiMiKi maps are based on data collected during May-July 2009. The Hinterlands maps are based on data collected during June-July 2010. FARDC is the acronym for the Armed Forces of the Democratic Republic of Congo, which merged with the CNDP (National Congress for the Defense of the People) in March 2009. FDLR is the acronym for the Democratic Forces for the Liberation of Rwanda. Mayi Mayi is an umbrella term for loosely affiliated groups of local militias. PNC is the acronym for the National Congolese Police. FRF is the acronym for the Forces Républicaines Fédéralistes.

Table 3: OLS Regression Estimates of Deforestation within Radius of Mines Before and After Dodd-Frank

	100m. radius	200m. radius	500m. radius
Constant	-0.005	-0.077	-0.024
	(0.869)	(0.151)	(0.743)
3Ts mine indicator	0.103*	0.142	0.127
	(0.098)	(0.180)	(0.404)
Indicator for policy territory	0.019	0.078	0.044
	(0.640)	(0.246)	(0.626)
Policy territory indicator *x* 3Ts mine indicator	-0.208***	-0.292**	-0.303*
	(0.006)	(0.016)	(0.074)
Percent forest loss over 2001-2007	-0.0007	-0.0001	-0.0122***
	(0.717)	(0.974)	(0.008)
Observations (mines)	659	659	659
Obs. with y = - 1 (slowing deforestation)	74	163	259
Obs. with y = +1 (accelerating deforestation)	58	120	230
F-Stat	2.78	2.08	3.55
Adjusted R^2	0.015	0.012	0.020

Notes: * p<0.10; ** p<0.05; *** p<0.01. The dependent variable is equal to -1 if deforestation within the radius around the centroid of the mine declined during 2011-2012, relative to 2008-2009. The dependent variable is equal to 1 if deforestation increased. The dependent variable is equal to 0 if there was not change in the rate of deforestation. Standard errors are clustered at the territory level. *P* values are shown in parentheses. The data come from a time-series analysis of satellite images that characterize global forest extent detailed in Hansen et al. (2013). Land area is divided into grid squares of approximately 30 meters on a side at the equator. Forest loss in each grid square is recorded as a binary event for each year, with the initial satellite image from 2000 taken as the base year. Grid boxes that fall partially within the relevant mine radius are considered to be entirely contained.

Table 4: Summary Statistics
(month-territory observations for 2004-2012 in territories of five eastern provinces)

Variable	*Mean*	*Std. Dev.*	*Min*	*Max*	*Description*
Time Variant					
Looting[a]	0.048	0.400	0	13	# of events described with text of loot, pillage, plunder, rob, steal, ransack, or seize
Battles[a]	0.242	1.351	0	36	# of battles between armed groups events
Civilians[a]	0.217	1.13	0	28	# of violence against civilians events
Recruiting[a]	0.007	0.136	0	7	# of events described with text of recruit, enlist, or draft
Looting indicator[a]	0.027	0.168	0	1	=1 if there was at least one looting event, otherwise =0
Battle indicator[a]	0.087	0.282	0	1	=1 if there was at least one battle event, otherwise =0
Civilians indicator[a]	0.092	0.289	0	1	=1 if there was at least one violence against civilian event, otherwise =0
Recruiting Indicator[a]	0.004	0.068	0	1	= if there was at least one recruiting event, otherwise =0
Policy indicator[b]	0.104	0.305	0	1	=1 starting in July 2010, for union of Section 1502 and mining ban territories,=0 otherwise
Mining ban[c]	0.017	0.126	0	1	=1 for territories in N. Kivu, S. Kivu, and Maniema during Sept. 2010-March 2011, =0 otherwise
Gold price[d]	1.950	0.815	0.91	3.49	World price of gold, normalized at 1 based on the January 2004 price, U.S. CPI adjusted
Tin price[d]	2.054	0.701	1.43	4.12	World price of tin, normalized at 1 based on the January 2004 price, U.S. CPI adjusted
Tantalum price[d]	1.422	0.713	0.90	2.83	World price of tantalum, normalized at 1 based on the January 2004 price, U.S. CPI adj.
Tungsten price[d]	3.475	0.554	2.93	4.80	World price of tungsten, normalized at 1 based on the January 2004 price, U.S. CPI adj.
Rainfall anomalies[e]	0.035	1.105	-2.66	3.59	Difference in rainfall and 1951-2012 average for month, divided by st. deviation
Adj. conflicts[a]	2.671	7.585	0	139	The sum of the # of conflict events (of any type excluding riots/protests) in all adjacent territories
Time Invariant					
Dry Season[e]	0.250	0.433	0	1	=1 for the three driest months in each territory, based on 1951-2012 precipitation averages
Wet Season[e]	0.250	0.433	0	1	=1 for the three wettest months in each territory, based on 1951-2012 precipitation averages
Gold mines[f]	5.786	11.030	0	69	# of gold mining sites or deposits
Cassiterite mines[f]	2.928	6.404	0	33	# of cassiterite mining sites or deposits
Coltan mines[f]	0.300	1.211	0	9	# of coltan (tantalum) mining sites or deposits
Wolframite mines[f]	0.114	0.622	0	5	# of wolframite (tungsten) mining sites or deposits

Notes: N=7560 for all variables, with 8 years, 12 months and 70 territories of observations. a) The source is the ACLED database. b) Takes a fractional value of 1/3 for July, 2010 because Dodd Frank was passed on July 21. For purposes here, 'Section 1502 territories' refers to territories on the U.S. State Department's map, which has been declassified and is available at: https://hiu.state.gov/Products/DRC_MineralExploitation_2011June14_HIU_U357.pdf. c) takes value of 2/3 for September 2010 and 1/3 for March 2011. d) The source is MetalPrices.com. e) The source is GPCC at http://kunden.dwd.de/GPCC/Visualizer. f) The source is the IPIS maps described in Spitaels and Hilgert (2008), Spittaels and Hilgert (2009), Spittaels (2010), and Spittaels and Hilgert (2010).

Table 5: Fixed Effects Estimates of Monthly Conflict Indicators

in territories of Five Eastern Provinces (2004-2012)

	Y= Looting Indicator						*Y= Battle Indicator*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Policy indicator	0.050*	0.071**	0.053**	0.072**	0.041*	0.060**	0.058	0.036	0.050	0.038	0.021	0.008
	(0.063)	(0.017)	(0.046)	(0.021)	(0.092)	(0.037)	(0.109)	(0.273)	(0.199)	(0.226)	(0.535)	(0.771)
Policy ind. *x* No. of 3T mines	-0.000	-0.002	-0.000	-0.002	-0.000	-0.002	0.000	-0.001	-0.000	-0.002	-0.000	-0.002
	(0.921)	(0.274)	(0.860)	(0.229)	(0.830)	(0.223)	(0.893)	(0.553)	(0.997)	(0.371)	(0.991)	(0.353)
Policy ind. *x* No. of gold mines	0.001	0.001	0.001	0.001	0.001	0.001	0.004***	0.002*	0.004***	0.003**	0.004***	0.002**
	(0.306)	(0.548)	(0.477)	(0.564)	(0.515)	(0.622)	(0.009)	(0.083)	(0.005)	(0.030)	(0.001)	(0.025)
Gold price *x* gold indicator			0.020**	0.001	0.017**	0.013			-0.001	-0.066	-0.007	-0.036
Tin price *x* cassiterite indicator			-0.003	-0.015	-0.001	-0.012			-0.004	-0.034	0.001	-0.026
Tant. price *x* coltan indicator			-0.005	0.008	-0.006	0.007			0.025	0.050	0.022	0.048
Tung. price *x* wolf. indicator			0.001	0.003	-0.001	0.002			-0.013	-0.034	-0.016	-0.036
Dry season indicator			0.002	0.002	0.000	0.000			0.006	0.007	0.003	0.004
Wet season indicator			-0.003	-0.003	-0.003	-0.003			0.006	0.007	0.007	0.008
Rainfall anomalies			0.000	0.002	-0.001	0.001			-0.004	-0.005	-0.006	-0.007
1 month lag rainfall anomalies			0.004	0.005*	0.003	0.005			-0.000	-0.001	-0.002	-0.003
2 month lag rainfall anomalies			0.003	0.005**	0.003	0.005**			0.007	0.006	0.006	0.005
Rainfall anomalies^2			0.000	-0.001	0.001	0.000			-0.002	-0.002	-0.001	-0.001
1 month lag rainfall anomalies^2			-0.003**	-0.003***	-0.003*	-0.003**			0.000	0.000	0.001	0.001
2 month lag rainfall anomalies^2			-0.003	-0.003*	-0.002	-0.003*			-0.003	-0.003	-0.002	-0.002
1 month lagged conflict					0.006**	0.005					0.016***	0.013***
2 month lagged conflict					0.002	0.001					0.007***	0.005*
3 month lagged conflict					0.003	0.002					0.005*	0.002
Adjacent territory conflict					0.002**	0.002**					0.003**	0.003**
1 month lagged adj. terr conflict					-0.000	-0.000					0.000	0.001
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R^2 (within)	0.052	0.083	0.055	0.085	0.068	0.092	0.053	0.099	0.055	0.102	0.086	0.121
Observations	7560	7560	7420	7420	7420	7420	7560	7560	7420	7420	7420	7420

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

Table 6: Fixed Effects Estimates of the Monthly Number of Conflict Episodes

in territories of Five Eastern Provinces (2004-2012)

	Y= Number of Looting Incidents						*Y= Number of Battle Incidents*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Policy indicator	0.156**	0.174***	0.177**	0.186***	0.144**	0.152**	0.251**	0.152	0.245*	0.182	0.054	-0.020
	(0.021)	(0.007)	(0.012)	(0.008)	(0.020)	(0.020)	(0.044)	(0.388)	(0.064)	(0.282)	(0.466)	(0.849)
Policy ind. *x* No. of 3T mines	-0.002	-0.004	-0.001	-0.003	-0.001	-0.003	0.003	-0.006	0.004	-0.001	0.005	0.000
	(0.580)	(0.288)	(0.776)	(0.395)	(0.710)	(0.392)	(0.714)	(0.483)	(0.607)	(0.843)	(0.383)	(0.975)
Policy ind. *x* No. of gold mines	0.001	0.001	0.000	0.001	-0.000	0.001	0.009	0.007	0.010*	0.011	0.007**	0.007*
	(0.686)	(0.705)	(0.926)	(0.671)	(0.921)	(0.761)	(0.148)	(0.151)	(0.093)	(0.111)	(0.046)	(0.077)
Gold price *x* gold indicator			0.032	-0.031	0.027	0.003			0.011	-0.594	-0.034	-0.380
Tin price *x* cassiterite indicator			-0.033	-0.061	-0.027	-0.053			-0.056	-0.226*	-0.024	-0.166*
Tant. price *x* coltan indicator			-0.011	0.003	-0.016	0.001			0.048	0.028	0.030	0.011
Tung. price *x* wolf. indicator			-0.001	-0.008	-0.005	-0.010			-0.012	-0.143*	-0.021	-0.141**
Dry season indicator			0.026**	0.027**	0.022*	0.022*			0.052	0.061	0.035	0.042
Wet season indicator			0.007	0.006	0.007	0.006			0.022	0.023	0.028	0.028
Rainfall anomalies			0.002	0.006	0.000	0.004			0.045**	0.045**	0.031*	0.030*
1 month lag rainfall anomalies			0.005	0.009	0.003	0.007			0.008	0.008	-0.005	-0.006
2 month lag rainfall anomalies			0.004	0.008**	0.003	0.007*			0.006	0.009	-0.003	-0.002
Rainfall anomalies^2			-0.004	-0.006*	-0.003	-0.005			-0.021**	-0.021**	-0.016*	-0.016*
1 month lag rainfall anomalies^2			-0.006**	-0.008**	-0.005*	-0.007**			-0.007	-0.007	0.000	-0.000
2 month lag rainfall anomalies^2			-0.006*	-0.008**	-0.004	-0.006*			-0.006	-0.007	0.002	0.001
1 month lagged conflict					0.019**	0.016**					0.139***	0.131***
2 month lagged conflict					0.005	0.002					0.025	0.019
3 month lagged conflict					0.003	0.000					0.067***	0.060**
Adjacent territory conflict					0.004*	0.004**					0.011	0.011*
1 month lagged adj. terr conflict					0.001	0.001					-0.002	-0.001
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R^2 (within)	0.044	0.068	0.047	0.071	0.063	0.081	0.042	0.067	0.043	0.070	0.118	0.129
Observations	7560	7560	7350	7350	7350	7350	7560	7560	7350	7350	7350	7350

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

Table 7: Fixed Effects Estimates of Monthly Conflicts, Allowing for Separate Policy Effects

in territories of Five Eastern Provinces (2004-2012)

	Y = Looting						*Y = Battles*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Y = Conflict Indicator Variables												
Policy Indicator	0.061***	0.069***	0.059**	0.073***	0.043**	0.056**	0.127***	0.080**	0.124***	0.089**	0.084**	0.046
	(0.004)	(0.003)	(0.015)	(0.008)	(0.046)	(0.029)	(0.001)	(0.035)	(0.004)	(0.012)	(0.022)	(0.132)
Mining Ban Indicator	0.004	-0.007	0.004	-0.003	0.019	0.011	-0.138***	-0.135**	-0.137***	-0.124**	-0.099**	-0.087*
	(0.874)	(0.778)	(0.869)	(0.889)	(0.397)	(0.629)	(0.008)	(0.012)	(0.006)	(0.018)	(0.029)	(0.072)
R^2 (within)	0.051	0.082	0.055	0.084	0.068	0.092	0.049	0.099	0.050	0.101	0.083	0.121
Panel B: Y = Number of Conflicts												
Policy Indicator	0.156***	0.170***	0.177***	0.191***	0.132**	0.142**	0.462***	0.317**	0.461***	0.432**	0.199**	0.131
	(0.004)	(0.003)	(0.008)	(0.006)	(0.021)	(0.032)	(0.000)	(0.048)	(0.002)	(0.022)	(0.015)	(0.151)
Mining Ban	-0.026	-0.053	-0.012	-0.030	0.029	0.010	-0.565***	-0.589***	-0.567***	-0.557***	-0.309**	-0.277**
	(0.733)	(0.508)	(0.873)	(0.721)	(0.688)	(0.896)	(0.004)	(0.003)	(0.004)	(0.007)	(0.014)	(0.028)
R^2 (within)	0.043	0.068	0.046	0.071	0.063	0.081	0.043	0.068	0.044	0.071	0.118	0.129
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged & adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Observations	7560	7560	7420	7420	7420	7420	7560	7560	7420	7420	7420	7420

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

Table 8: Fixed Effects Estimates of Monthly Conflicts Across the Entire Policy Region

in territories of Five Eastern Provinces (2004-2012)

	Y = Looting						*Y = Battles*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: ***Y = Conflict Indicator Variables***												
Policy Indicator	0.061***	0.068***	0.060**	0.072***	0.047**	0.059**	0.104***	0.048	0.099**	0.057**	0.066*	0.023
	(0.002)	(0.002)	(0.013)	(0.007)	(0.033)	(0.019)	(0.002)	(0.126)	(0.012)	(0.049)	(0.051)	(0.350)
R^2 (within)	0.051	0.082	0.055	0.084	0.068	0.092	0.051	0.101	0.052	0.103	0.084	0.122
Panel B: ***Y = Number of Conflicts***												
Policy Indicator	0.151***	0.157***	0.175***	0.184***	0.138**	0.145**	0.367***	0.179	0.361***	0.291*	0.142*	0.058
	(0.002)	(0.001)	(0.005)	(0.005)	(0.012)	(0.019)	(0.001)	(0.257)	(0.006)	(0.093)	(0.059)	(0.531)
R^2 (within)	0.043	0.068	0.046	0.071	0.063	0.081	0.041	0.066	0.042	0.070	0.118	0.129
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged & adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Observations	7560	7560	7420	7420	7420	7420	7560	7560	7420	7420	7420	7420

Notes: * $p<0.10$; ** $p<0.05$; *** $p<0.01$. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

Table 9: Fixed Effects Estimates of Monthly Violence Against Civilians

in territories of Five Eastern Provinces (2004-2012)

	Y = Violence Against Civilian Indicator						*Y = Number of Violence Against Civilian Episodes*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Policy Interacted with Mines												
Policy Indicator	0.053	0.144***	0.072*	0.145***	0.033	0.104**	0.472**	0.756***	0.509***	0.733***	0.292**	0.498***
	(0.224)	(0.001)	(0.091)	(0.002)	(0.313)	(0.002)	(0.013)	(0.003)	(0.009)	(0.004)	(0.026)	(0.004)
Policy ind. *x* No. of 3T mines	0.001	-0.001	-0.000	0.001	0.000	-0.000	-0.002	-0.007	-0.003	-0.006	-0.003	-0.005
	(0.778)	(0.484)	(0.858)	(0.821)	(0.835)	(0.865)	(0.829)	(0.319)	(0.762)	(0.351)	(0.632)	(0.406)
Policy ind. *x* No. of gold mines	0.004*	0.001	0.002	0.001	0.002	0.001	0.006	0.001	0.003	0.006	0.001	0.003
	(0.083)	(0.487)	(0.170)	(0.355)	(0.138)	(0.476)	(0.360)	(0.796)	(0.532)	(0.329)	(0.830)	(0.519)
R^2 (within)	0.062	0.120	0.071	0.124	0.121	0.156	0.049	0.093	0.056	0.099	0.162	0.179
Panel B: Separate Policy Indicators												
Policy Indicator	0.105***	0.158***	0.104***	0.169***	0.050	0.109***	0.551***	0.800***	0.563***	0.847***	0.263**	0.496***
	(0.007)	(0.000)	(0.014)	(0.001)	(0.123)	(0.002)	(0.000)	(0.002)	(0.001)	(0.003)	(0.012)	(0.002)
Mining Ban Indicator	-0.027	-0.055	-0.015	-0.044	0.037	0.009	-0.228	-0.340*	-0.154	-0.303*	0.141	0.020
	(0.494)	(0.181)	(0.681)	(0.271)	(0.260)	(0.792)	(0.163)	(0.053)	(0.323)	(0.092)	(0.252)	(0.877)
R^2 (within)	0.058	0.120	0.069	0.124	0.120	0.156	0.049	0.094	0.056	0.099	0.162	0.179
Panel C: Single Policy Indicator												
Policy Indicator	0.100***	0.145***	0.098**	0.156***	0.054*	0.110***	0.513***	0.720***	0.539***	0.774***	0.291***	0.507***
	(0.007)	(0.000)	(0.014)	(0.000)	(0.087)	(0.000)	(0.000)	(0.002)	(0.001)	(0.003)	(0.004)	(0.001)
R^2 (within)	0.058	0.120	0.069	0.124	0.120	0.156	0.048	0.093	0.055	0.099	0.162	0.179
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged & adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Observations	7560	7560	7420	7420	7420	7420	7560	7560	7420	7420	7420	7420

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

Table 10: Fixed Effects Estimates of Monthly Militia Recruiting Episodes

in territories of Five Eastern Provinces

	Y = Recruiting Indicator, Sample is 2004-2012						*Y = Recruiting Indicator, Sample is 2004-2011*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Policy Interacted with Mines												
Policy Indicator	0.013	0.005	0.011	0.004	0.007	-0.000	-0.001	-0.003	-0.004	-0.004	-0.005	-0.005
	(0.215)	(0.412)	(0.336)	(0.554)	(0.439)	(0.979)	(0.864)	(0.543)	(0.351)	(0.509)	(0.321)	(0.518)
Policy ind. *x* No. of 3T mines	0.000	0.000	0.000	0.001	0.000	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(0.643)	(0.294)	(0.858)	(0.341)	(0.855)	(0.335)	(0.138)	(0.109)	(0.225)	(0.152)	(0.228)	(0.154)
Policy ind. *x* No. of gold mines	-0.000	0.000	-0.000	0.000	-0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.635)	(0.844)	(0.627)	(0.729)	(0.530)	(0.942)	(0.517)	(0.355)	(0.307)	(0.332)	(0.313)	(0.323)
R^2 (within)	0.026	0.047	0.026	0.048	0.036	0.055	0.020	0.027	0.022	0.029	0.023	0.030
Panel B: Separate Policy Indicators												
Policy Indicator	0.011	0.005	0.007	0.004	0.002	-0.002	-0.001	-0.004	-0.009**	-0.007	-0.010**	-0.008
	(0.127)	(0.346)	(0.436)	(0.504)	(0.790)	(0.766)	(0.624)	(0.184)	(0.048)	(0.149)	(0.034)	(0.139)
Mining Ban Indicator	0.015	0.014	0.016	0.013	0.021	0.018	0.028*	0.029*	0.029*	0.028*	0.030*	0.029*
	(0.419)	(0.456)	(0.395)	(0.503)	(0.230)	(0.303)	(0.095)	(0.091)	(0.089)	(0.089)	(0.083)	(0.082)
R^2 (within)	0.026	0.047	0.027	0.048	0.037	0.055	0.021	0.029	0.024	0.031	0.025	0.032
Panel C: Single Policy Indicator												
Policy Indicator	0.014**	0.009*	0.010	0.007	0.006	0.003	0.007	0.005	0.000	0.002	-0.000	0.002
	(0.043)	(0.094)	(0.232)	(0.275)	(0.404)	(0.699)	(0.174)	(0.366)	(0.989)	(0.739)	(0.941)	(0.776)
R^2 (within)	0.025	0.047	0.026	0.048	0.036	0.055	0.018	0.026	0.021	0.029	0.022	0.030
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged & adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Observations	7560	7560	7420	7420	7420	7420	6720	6720	6580	6580	6580	6580

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

Appendix: Constructing the Territory Level Rainfall Variables

We estimated monthly precipitation amounts for the 150 territories of the DRC using the following process. First, we downloaded precipitation normals from GPCC Visualizer (http://kunden.dwd.de/GPCC/Visualizer) as ascii ArcView GRID files, and then converted to rasters using the ArcGis ascii to raster tool. Next we downloaded error corrected monthly precipitation data as a NetCDF file. This file contained monthly precipitation data from 1901-2010 and had to be unpackaged to obtain the years relevant for our empirical analysis. The process to unpackage the file and convert individual months to ArcGis raster files was done with the help of a code written in Python. We next used the zonal statistics tool in ArcGis 10.2 to calculate average precipitation values for each of the 150 territories. We resampled the precipitation data to 0.1 degree resolution for this purpose, because in some cases the 1 degree pixels of precipitation data were larger than the territories. After resampling, we were able to calculate average precipitation values for all territories.

The precipitation data for 2004-2010 was already error corrected in GPCC raw data files, but we needed to correct the 2011 and 2012 data for systematic gauge errors prior to use, so that it would correspond to the 2004-2010 data. We executed the correction using the GPCC 1 degree relative systematic gauge error product, available for every month during 2011-2012 period from GPCC Visualizer. We converted the percent error to a multiplication factor which we applied to each month of the 2011-2012 precipitation grids. After this correction was achieved, we resampled the data to 0.1 degrees using the same procedure described above.

Table A1: Robustness of Table 5 Policy Variable Coefficients

in territories of Five Eastern Provinces (2004-2012)

	Y= Looting Indicator						*Y= Battle Indicator*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Omits Urban Areas												
Policy indicator	0.060**	0.070**	0.063**	0.071**	0.049**	0.059*	0.084**	0.045	0.076**	0.042	0.040	0.009
Policy ind. *x* No. of 3T mines	-0.000	-0.001	-0.000	-0.002	-0.000	-0.002	-0.001	-0.002	-0.001	-0.002	-0.001	-0.002
Policy ind. *x* No. of gold mines	0.001	0.001	0.001	0.001	0.001	0.001	0.004**	0.002*	0.004***	0.002**	0.004***	0.002**
R^2 (within)	0.053	0.083	0.057	0.085	0.069	0.093	0.059	0.105	0.060	0.107	0.093	0.128
Observations	7128	7128	6930	6930	6930	6930	7128	7128	6930	6930	6930	6930
B. Min. Prices x No. of Mines												
Policy indicator			0.053*	0.067**	0.042*	0.055**			0.051	0.052	0.025	0.021
Policy ind. *x* No. of 3T mines			-0.001	-0.002	-0.001	-0.002			-0.000	0.000	-0.000	0.000
Policy ind. *x* No. of gold mines			-0.001	0.001	-0.001	0.001			0.005*	0.002*	0.004*	0.002
R^2 (within)			0.056	0.085	0.069	0.092			0.055	0.100	0.087	0.121
Observations			7420	7420	7420	7420			7420	7420	7420	7420
C. Lagged Dep. Variables												
Policy indicator					0.038*	0.057**					0.025	0.020
Policy ind. *x* No. of 3T mines					-0.000	-0.002					-0.000	-0.002
Policy ind. *x* No. of gold mines					0.001	0.001					0.003***	0.002**
R^2 (within)					0.075	0.095					0.093	0.120
Observations					7420	7420					7420	7420
D. Omits Policy Neighbors												
Policy indicator	0.056**	0.075**	0.058**	0.076**	0.045*	0.061**	0.060	0.043	0.061	0.047	0.030	0.011
Policy ind. *x* No. of 3T mines	-0.000	-0.002	-0.000	-0.002	-0.000	-0.002	0.000	-0.001	0.001	-0.002	0.000	-0.002
Policy ind. *x* No. of gold mines	0.001	0.001	0.001	0.001	0.001	0.001	0.004***	0.002*	0.004***	0.003**	0.004***	0.002**
R^2 (within)	0.062	0.089	0.065	0.091	0.081	0.100	0.064	0.112	0.065	0.115	0.102	0.138
Observations	5724	5724	5618	5618	5618	5618	5724	5724	5618	5618	5618	5618
Territory fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged and adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors (not shown) are clustered at the territory level. Panel A omits the four territories that are comprised entirely of cities which are Lubumbashi in Katanga (population 1,786,397), Goma in North Kivu (population of 1,000,000), Kisangani in Orientale (population 925,977) and Bukavu in South Kivu (806,940). Panel B includes interactions with the number of mines and world mineral prices, rather than mine indicator variables employed in table 5. Column C employs lags of the dependent variable, rather than the lagged number of conflicts of all types, which is the measure employed in the baseline. Panel D omits 17 territories that are adjacent to at least one policy territory.

Table A2: Robustness of Table 6 Policy Variable Coefficients

in territories of Five Eastern Provinces (2004-2012)

	Y= Looting Indicator						Y= Battle Indicator					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Omits Urban Areas												
Policy indicator	0.180**	0.181**	0.207***	0.197**	0.168**	0.159**	0.358***	0.152	0.358***	0.187	0.110	-0.042
Policy ind. *x* No. of 3T mines	-0.003	-0.004	-0.002	-0.003	-0.002	-0.003	-0.001	-0.006	0.003	-0.002	0.003	0.000
Policy ind. *x* No. of gold mines	0.000	0.001	-0.000	0.001	-0.001	0.001	0.007	0.007	0.007	0.010	0.006*	0.007*
R^2 (within)	0.045	0.069	0.048	0.072	0.065	0.082	0.046	0.069	0.048	0.072	0.123	0.132
Observations	7128	7128	6930	6930	6930	6930	7128	7128	6930	6930	6930	6930
B. Min. Prices x No. of Mines												
Policy indicator			0.151**	0.161**	0.121**	0.127**			0.205*	0.183	0.037	-0.020
Policy ind. *x* No. of 3T mines			-0.004	-0.006	-0.005	-0.006			0.004	0.004	0.004	0.005
Policy ind. *x* No. of gold mines			-0.002	0.003	-0.002	0.002			0.013	0.009	0.009	0.006
R^2 (within)			0.050	0.071	0.066	0.082			0.044	0.068	0.119	0.128
Observations			7420	7420	7420	7420			7420	7420	7420	7420
C. Lagged Dep. Variables												
Policy indicator					0.137**	0.151**					0.119	0.101
Policy ind. *x* No. of 3T mines					-0.001	-0.003					0.003	-0.001
Policy ind. *x* No. of gold mines					0.000	0.001					0.006*	0.006*
R^2 (within)					0.063	0.082					0.130	0.140
Observations					7420	7420					7420	7420
D. Omits Policy Neighbors												
Policy indicator	0.167**	0.181***	0.192**	0.196***	0.155**	0.154**	0.260**	0.210	0.281**	0.259	0.056	-0.017
Policy ind. *x* No. of 3T mines	-0.002	-0.004	-0.001	-0.003	-0.001	-0.003	0.003	-0.006	0.006	0.000	0.006	0.002
Policy ind. *x* No. of gold mines	0.001	0.001	0.000	0.001	-0.000	0.001	0.009	0.007	0.008	0.011	0.006*	0.007
R^2 (within)	0.053	0.076	0.056	0.079	0.075	0.090	0.049	0.074	0.050	0.077	0.139	0.148
Observations	5724	5724	5618	5618	5618	5618	5724	5724	5618	5618	5618	5618
Territory fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged and adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors (not shown) are clustered at the territory level. Panel A omits the four territories that are comprised entirely of cities which are Lubumbashi in Katanga (population 1,786,397), Goma in North Kivu (population of 1,000,000), Kisangani in Orientale (population 925,977) and Bukavu in South Kivu (806,940). Panel B includes interactions with the number of mines and world mineral prices, rather than mine indicator variables employed in table 6. Column C employs lags of the dependent variable, rather than the lagged number of conflicts of all types, which is the measure employed in the baseline. Panel D omits 17 territories that are adjacent to at least one policy territory.

Table A3: Placebo Tests of False Policy Treatment Dates Prior to Dodd Frank

in territories of Five Eastern Provinces (January 2004- June 2010)

	July 2009 – June 2010 Treatment Placebo				January 2009 – June 2010 Treatment Placebo (to correspond with Kimia II)				July 2008 - June 2010 Treatment Placebo			
	Loot	Loot	Battle	Battle	Loot	Loot	Battle	Battle	Loot	Loot	Battle	Battle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Y = Conflict Indicator												
Policy indicator placebo	-0.011	-0.010	-0.015	0.000	-0.017	-0.016	0.003	0.016	-0.004	-0.005	0.050	0.052*
	(0.353)	(0.377)	(0.572)	(0.999)	(0.101)	(0.104)	(0.878)	(0.415)	(0.752)	(0.714)	(0.153)	(0.087)
Placebo x No. of 3T mines	0.001	0.001	0.003	0.003	0.001	0.001	0.002	0.002	-0.002	-0.001	0.020	0.024
	(0.342)	(0.336)	(0.365)	(0.351)	(0.296)	(0.290)	(0.343)	(0.313)	(0.919)	(0.964)	(0.709)	(0.602)
Placebo x No. of gold mines	0.000	0.000	0.002	0.002	0.000	0.000	0.001	0.001	-0.000	-0.000	0.000	0.000
	(0.767)	(0.729)	(0.313)	(0.264)	(0.555)	(0.477)	(0.386)	(0.300)	(0.481)	(0.556)	(0.981)	(0.781)
R^2 (within)	0.021	0.025	0.035	0.065	0.021	0.026	0.034	0.064	0.021	0.025	0.036	0.066
Observations	5320	5320	5320	5320	5320	5320	5320	5320	5320	5320	5320	5320
Panel B: Y = Conflict Incidents												
Policy indicator placebo	-0.013	-0.010	-0.062	0.020	-0.022	-0.019	-0.057	0.008	-0.001	-0.002	0.125	0.126
	(0.544)	(0.617)	(0.647)	(0.767)	(0.201)	(0.216)	(0.628)	(0.891)	(0.954)	(0.937)	(0.495)	(0.349)
Placebo x No. of 3T mines	0.001	0.001	0.008	0.006	0.002	0.002	0.005	0.004	-0.006	-0.005	0.046	0.081
	(0.373)	(0.364)	(0.468)	(0.415)	(0.241)	(0.230)	(0.534)	(0.483)	(0.817)	(0.835)	(0.828)	(0.579)
Placebo x No. of gold mines	0.013	0.010	0.003	-0.022	0.000	0.000	0.003	0.004	-0.000	-0.000	-0.002	-0.000
	(0.185)	(0.261)	(0.894)	(0.241)	(0.616)	(0.548)	(0.550)	(0.295)	(0.613)	(0.726)	(0.624)	(0.946
R^2 (within)	0.019	0.038	0.038	0.107	0.020	0.038	0.038	0.107	0.019	0.038	0.038	0.108
Observations	5320	5320	5320	5320	5320	5320	5320	5320	5320	5320	5320	5320
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 72)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
World mineral price controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rainfall controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged and adj. conflict controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: * p<0.10; ** p<0.05; *** p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.